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TYPES OF DEXTRALITY AND THEIR IMPLICATIONS[1]

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The conventional classification of individuals into right-handed and left-handed furnishes very little information about their manual habits. Yet detailed information concerning the way in which the hands are used has considerable practical importance and may possibly throw light upon certain theoretical problems related to speech, hand-writing, nervous stability, and temperament.

It has been my observation that all degrees of unidextrality (the use of one hand in preference to the other) exist and I have sought for some simple method of determining the degree of such unidextrality without recourse to time consuming individual tests. The most fertile suggestion concerning dextrality types that I have seen is that of Rife.[2] I have classified some 1500 individuals according to Rife's scheme which has been slightly modified by myself. In addition to the handedness formula I have obtained other data, such as the sighting eye, the spade foot, the thumb uppermost when the two hands are clasped with fingers interclasping and the like. I have sought to determine whether dextrality types are tied up with degrees of unidextrality or, even, with psychic patterns.

(1) *Handedness, inherited or acquired?* The statistical survey outlined above has value quite apart from the problem of the origin of handedness in native or acquired tendencies. If handedness is socially conditioned it may be well to ask what society achieves by its dictates and whether it might use its power in conditioning manual reactions more effectively. Moreover, a survey of manual habits as they exist in society as organized at present may sharpen our observation in genetic studies, although it cannot, of course, take the place of such genetic study.

[1]This investigation was made possible by a grant from the National Research Council through its Committee on Scientific Problems of Human Migration.

[2]J. M. Rife, Types of dextrality, *Psychol. Rev.*, 29, 1922, 474-480.

Watson has recently stated very clearly his conviction that right-handedness is a habit socially instilled. "Our whole group of results on handedness leads us to believe," he writes, "that there is no fixed differentiation of response in either hand until social usage begins to establish handedness. Society soon thereafter steps in and says 'Thou shalt use thy right hand.' Pressure promptly begins. 'Shake hands with your right hand, Willy.' We hold the infant so that it will wave 'bye bye' with the right hand. We force it to eat with the right hand. This in itself is a potent enough conditioning factor to account for handedness."[3] "The main problem is, I believe, settled: handedness is not an 'instinct.' It is possibly not even structurally determined. It is socially conditioned. But why we have 5% of out and out left-handers and from 10-15% who are mixtures—*e.g.* using the right hand to throw a ball, write or eat, but the left hand to guide an axe or hoe, etc.—is not known."[4]

The present paper makes little contribution to the solution of the nature *versus* nurture problem, although I believe that little is in favor of a native factor operating in manual preferences. Watson asks for structural differences as basal to functional differences and he is not apparently willing to concede that a little difference in structure may make a great difference in function. Later I shall cite some evidence of structural differences between the extremely right and left-handed but at present I am willing to leave open the question of nature versus nurture. But, to repeat, the present statistical survey furnishes a number of suggestions concerning lines of approach in a genetic study. It suggests also that it is necessary in studying infants or even apes to consider each case on its own merits.[5] If there is only a small percentage of extremely right or left-handed and a large percentage of mild cases the former may be obscured by a massed treatment.

(2) *Dextrality Types.* Rife states "there are at least three distinct types of right-handedness, with three corresponding types of left-handedness, making in all 6 forms of dextrality." These types of dextrality are determined by observation of the use of the hands in unimanual operations such as throwing, writing, eating and the like, and bimanual activities such as batting, sweeping, spading, and using the axe. In bimanual operations handedness is determined, says Rife, by the hand "nearer the business end of the instrument."

For many individuals the bimanual preference is the opposite of their unimanual preference. They throw, for example, right-handed, but they bat left-handed and they use a broom or shovel left-handed, that is, with the left hand placed below the right. Other individuals have a divided bimanual preference. They bat right-handed but they use a broom or shovel left-handed.

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[3] J. B. Watson, What the nursery has to say about instincts, *Ped. Sem.*, 32, 1925, 321

[4] *Op. cit.*, 322.

[5] As was done by Wolfgang Köhler in his book *The Mentality of Apes*, English trans., 1925.

I have followed Rife's classification with a few additions. The following formulae are used for the various types. If the right hand is the preferred hand in both unimanual and bimanual activities, an individual is labelled RRR. Such a right-handed person uses the right hand for eating, writing, throwing, etc., bats right-handed and places the right hand below the left in sweeping and spading.

If an individual is right-handed in unimanual activities but has an opposite preference in bimanual activities he is labelled RLL. He writes, throws, eats, etc. right-handed but he bats and sweeps left-handed.

If an individual is right-handed for unimanual activities but has a divided preference in bimanual activities, that is, bats right-handed but sweeps left-handed, he is labelled RRL.

The corresponding formulae for the left-handed are LLL, LRR, and LLR. The LLL individual is left-handed for both unimanual and bimanual activities, that is he both throws left-handed and bats and sweeps left-handed. The LRR man is left-handed for unimanual operations but right-handed for bimanual activities; he throws and writes left-handed but bats and sweeps right-handed. The LLR type is left-handed in one-handed operations and has a divided preference in the bimanual activities, for example, is left-handed in batting but right-handed in sweeping.

So far I have followed Rife. There are, however, refinements in observation that extend the list of formulae somewhat. Let us consider, for example, not only the relative position of the hands in sweeping but also the movements by which the broom is manipulated. There are individuals who place the left hand below the right on the broomstick and who are called left-handed sweepers but who employ the left hand merely to steady the broom; the right hand exerts the pressure and does the guiding. I have labeled this group RRL_1. I believe this group sweeps mainly toward the left but there are many exceptions to this statement.

There are other individuals who not only place the left hand below the right on the broomstick but exert the main pressure on the left; the right hand steadies the implement. I have labeled this group RRL_3. In shifting the direction in which they sweep they may reverse the movement used by the left hand; there is a push toward the right or a pull toward the left.

Sweepers of both the RRL_1 and RRL_3 types, instead of shifting from push to pull in manipulating a broom, may revolve the body as they work. One sees frequent illustrations of this, a rhythmic revolution of the whole body of which the sweeper is scarcely conscious.

There is still a third type of sweeper, the sweeper who reverses the relative position of the two hands, now placing the left hand below the right and now the right below the left. I have labeled this individual RRR-L. He may be an individual who has little preference as to which hand takes the initiative in sweeping, and to this extent is ambidextral. Boys who have done a considerable amount of janitor's work tell me they alternate the hands in this way in order to avoid fatigue.

The RRR individual who sweeps right-handed, that is, with the right hand below the left on the broomstick, should, theoretically, afford as great variety of subtypes as the left-handed sweepers. I have found, however, only a few cases in which the right-handed sweeper thought that the left hand was doing the work, but there are a few, labeled RRR_4.

Among left-handed individuals of the type LLR, it is also possible to distinguish those who report that the left hand does the work in spite of the dominant position of the right hand, and those who report that the right hand exerts the strength. I have, however, made no attempt to sort out these individuals since my left-handed groups are so small.

It may be said that observation of the way in which a broom is wielded is particularly enlightening since there appears to be no particular social pressure exerted (apart from unconscious imitation) to make one adopt a particular method of manipulation. Many individuals are completely unaware of how they use a broom and are obliged to handle one in order to report their method. But their procedure may be none the less highly stabilized. To get some idea of the time of such stabilization it is necessary to make observations on young children. Records which I have from kindergarten children indicate that the procedure is frequently stabilized by five years of age, in which case it is difficult to think of such stabilization as an outcome of practice or habit, since five-year olds have little serious experience with sweeping.

Batting habits are probably much more influenced by social pressure than are broom habits. Boys, particularly, are subjected to this pressure. In determining the formula for batting some difficulty arises in the relatively infrequent cases where the wrists are crossed. In classification of individuals I have consistently used the relative position of the two hands as the deciding factor. Thus, an individual whose left hand is above the right as the bat is raised is classified as left-handed in batting even though he swings the bat over the right shoulder and as a right-handed batter if the right hand is above the left and the bat swung over the left shoulder.

There are reports of mild bimanual preference; there are also reports of mild unimanual preference. In such cases social pressure usually determines that such individuals shall use the right hand in the conventional way but under certain circumstances, such, for example, as an accident to the right hand in childhood or imitation of a left-handed relative in the home, they may use the left hand or show an inconstant preference, use, for example, the left hand for eating and the right hand for throwing.

These cases of mild preference in the use of the hands raise the question of the meaning of ambidextrality. Among my 1500 cases I have perhaps 5 or 6 which might be labeled ambidextral in the sense that there exists scarcely any indication of a preferential use of the hands. Either eye,

moreover, may be the fixating eye and when the hands are clasped with the fingers alternating, now the right and now the left thumb is uppermost—a very rare phenomenon. To repeat, cases of ambidextrality in this sense are exceedingly rare. There is, however, another use of the term. Individuals who belong to the type RLL or LLR frequently describe themselves as 'ambidextrous.' It will be recalled that for these individuals the bimanual preference is opposite to the unimanual. They throw right-handed and bat left-handed or the reverse. Among men this group is a fairly large one—about 14 out of 100 cases fall into it. Among 100 women not more than 7 or 8 cases occur. This group is a most interesting one as will be seen from later discussions.

It will also appear from later discussions that the two ambidextral types described above seem to be quite unlike so far as nervous or emotional stability is concerned. The first type, in which no fixation of function is evident, is a curiously unstable type, a statement which does not apply in the least to the second group.

A possible formula, which Rife asserts is never found except when natural inclinations have been tampered with, is RLR, or in the case of the left-handed, LRL. An RLR individual is one who is right-handed in unimanual operations, bats left-handed, and is right-handed in sweeping or spading, that is, he is left-handed in a difficult bimanual performance but right-handed in a minor one. The reverse is true for an LRL person. The latter is left-handed in unimanual operations, a right-handed batter, and a left-handed sweeper.

My results confirm Rife's. Only 15 individuals out of a total of 1251 gave formula RLR. This observation is an important one since it points to a native as against an acquired preference in the use of the hands, for it would seem that in the acquisition of social habits, accidentally conditioned—at least in certain bimanual forms—all or any combinations might occur.

(3) *Statistical Survey of Dextrality Types.* Some 1280 right-handed persons and some 250 left-handed have contributed to the data presented in this and following sections of the paper. Before presenting the tabulated material, it will be necessary to describe the[6] way in which the material was collected and to comment on certain sources of error. Questionaries so worded as to elicit information about matters of fact, not of opinion, were sent to the members of the American Psychological Association, whose coöperation I most gratefully acknowledge.[6]

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[6]The questionary in somewhat abbreviated form ran as follows:

QUESTIONARY ON DEXTRALITY

(A) Name..... Date..... Man or Woman.....
(B) Are you right or left-handed? Or ambidextrous?.....
(C) Check which hand you throw a ball with. Right ( )—Left ( ). Which hand do you write with? R ( )—L ( ). Which hand do you hammer with? R ( )—L ( ). Do you swing a baseball bat over your right or left shoulder? R ( )—L ( ).
(D) Close your eyes. Clasp your hands with fingers interlacing. Which thumb is outside?..... Repeat. Which thumb is outside?.....
(E) Fold your arms. Which arm is uppermost?..... Repeat. Which arm?.....
(F) Encircle one of the following numbers to show the way in which your ordinarily employ your hands in using a broom.
  (1) Right hand on top, left hand at center of broom stick. Work done primarily by right hand.
  (2) Left hand at top, right hand at center of broom stick. Work done primarily by right hand.
  (3) Right at top, left hand at center. Work done primarily by left.

(Continued on page 322)

From this source I obtained 251 adequately filled out returns; 216 from men, 35 from women. Many of the women psychologists had never used a baseball bat or a golf club so that it was impossible to determine from their records their handedness formula. The questionary returns from the psychologists bear every evidence of having been carefully and expertly filled out and have been accepted at their face value except in some instances in the matter of the sighting eye or spade foot to which reference will be made later.

A few additional questionaries were also obtained from scientists, other than psychologists, and from professional men. In particular, the questionary was used in the collection of information from left-handed individuals. In a number of these cases it was necessary to follow up the questionary by a letter asking for more detailed information or for a rechecking of the report. These questionaries are probably less accurately filled out than those returned by psychologists.

Questionaries have also been filled out by some 500 college and high school students, but in these instances, the questionaries have been personally checked by an examiner.

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(4) Left hand at top, right hand at center. Work done by left hand.

Encircle one of the following numbers to show the way in which you ordinarily employ your hands in using a baseball bat.

(5) Left hand at end of bat, nearest the body. Right hand toward the center of bat. Bat extended to the right.
(6) Right hand at end of bat, nearest the body. Left hand toward the center of the bat. Wrists crossed. Bat extended to the right.
(7) Right hand at end of bat, nearest the body. Left hand toward the center of the bat. Bat extended to the left.
(8) Left hand at end of bat, nearest the body. Right hand toward the center of the bat. Wrists crossed. Bat extended to the left.

In the same manner draw a circle about the number that indicates how you use your hands in manipulating the following: Refer to either the illustration of the broom or the bat.

Axe	1	2	3	4	5	6	7	8	Golf-club	1	2	3	4	5	6	7	8
Rake	1	2	3	4	5	6	7	8	Spade	1	2	3	4	5	6	7	8
Shovel	1	2	3	4	5	6	7	8	Pitchfork	1	2	3	4	5	6	7	8

(G) Which foot do you use on a spade? Right ( )—Left ( ). Which stocking do you put on first? R ( )—L ( ).

(H) Were you slow in learning to talk?..... Have you ever had a speech disturbance?..... What kind?.....

(I) Is your right or left eye the sighting eye, if either?.....
Can you use your non-preferred eye in a microscope?.....

(J) Try the following experiment. Use a small finger-ring; hold it at arm's length, and encircle a small object a few yards away. Focus on the object. Now close your right eye. Check what happens.
(A) No change
(B) Ring moves to right of object
(C) Ring moves to left of object
Refocus. Close your left eye. What happens?
(A) No change
(B) Ring moves to right of object
(C) Ring moves to left of object
Repeat this test until you are confident of the accuracy of your report.

(K) Are there any left-handed persons in your immediate family?.....
Yes ( )—No ( ). Name the relationship.....

(L) Have you any left-handed relatives, near or remote, on your father's side? Yes ( )—No ( ). If so, name the relationship.....
.....

(M) Have you any left-handed relatives, near or remote, on your mother's side? Yes ( )—No ( ). If so, name the relationship.....
.....

(N) Write your name and address and age with your right hand, below.
(O) With your left hand, below.

When the questionary was not usable, cards with blanks for record of the desired information were filled out by examiners who had actually tested the subjects in their manual habits and tried out the sighting eye. This procedure was, of course, necessary in handling delinquent and feeble-minded groups.[7]

Although the work has been done carefully there are several sources of error, apart from individual inaccuracy in report. It is obviously not always an easy thing to determine which hand does the most work in manipulating the broom. Formulae RRL_1 and RRL_3 certainly overlap to some extent.

Another difficulty was encountered in determining the batting habits of women and girls. Their information concerning this matter was very limited as compared with that of the men, except in the case of the young college women who play baseball.

The questionary as first formulated and the instructions sent out to collaborators made no reference to the possibility of the relative position of the hands being shifted in the use of the broom and various tools. My attention was called to this possibility by returns received from the members of the American Psychological Association. It is probably of some importance in determining subdivisions of the various classes.

Tables I and II give the distribution of handedness formulae, RRL_1 and RRL_3 being grouped together. Table I for 1287 cases in which all types of dextrality were included, and Table II for 251 left-handed individuals.[8] In Table I the percent of left-handedness was determined solely by the number which were found in groups in which the selection of left-handers played no part. For Table II a definite effort was made to secure as many records as possible from left-handed individuals. If, on the average, 5% of the population are left-handed, Table II represents a survey of about 5000 individuals.

In Table I there is a fourfold classification. The percentages for men and women are entered separately and, under each, percentages for superior and inferior groups. The latter classification is only a rough one. The superior group includes professional and college men and women, high school and public school boys and girls. The inferior group includes adult and

[7] I am indebted to the following individuals for their help in obtaining data on which Tables I-XXVI are in part based: Mr. L. Yepsen (127 cases, Vineland, N. J.); Miss Agnes Hill (100 institutional cases, Mt. Morrison, Colorado); Miss Lillian Portenier (52 high school students, University of Wyoming); Dr. Richard S. Uhrbrock (161 penitentiary inmates; 89 Whittier School Boys; 125 Indians; 49 inmates of the Wyoming State Training School; and 34 inmates of the Wyoming State Hospital); Miss Winnie Wheeler (139 Indians and 39 Chinese). A number of records on left-handed individuals have been sent me by Miss Beatrice McLeod and by Miss Hazel Cushing of the State Department of Education, Wyoming. Those who collected material have been enabled to do so by the kindness of the superintendents in charge of the institutions concerned. We take this opportunity to express our thanks to Dr. J. Harold Williams, Dr. E. Sullivan, Mr. Knox, and Miss Mahon for their help with the California subjects; and to Dr. C. T. Jones former Superintendent of the Wyoming State Training School; Dr. C. H. Solier, Superintendent Wyoming State Hospital; Mr. R. P. Haase, Superintendent Shoshone Reservation, Wyoming; and Mrs. Cornelia B. Mills, former Secretary of the Wyoming State Board of Charities and Reform. I am also indebted to Miss Reba Davis, Librarian of the University of Wyoming, for her help in obtaining library material.

[8] Records from Indians and Chinese are not included.

juvenile delinquents, inmates of institutions for the feeble-minded and insane, and boys and girls in special classes for sub-normal children. Some of the public school children included in the superior group are possibly below average in intelligence or character.

TABLE I
Handedness Formulae, 1287 Cases

Formula	Men				Women			
	Superior Group		Inferior Group		Superior Group		Inferior Group	
	No.	% of Total	No.	% of Total	No.	% of Total	No.	% of Total
RRR	173	41.09	150	35.62	110	36.66	47	32.41
RRL	146	34.67	194	46.08	154	51.33	79	54.48
RLL	73	17.33	42	9.97	26	8.66	8	5.51
RLR	3	.71	11	2.61	0	0	1	.68
Left-Handed	26	6.17	24	5.70	10	3.33	10	6.89
Total all Formulae	421	99.97	421	99.98	300	99.98	145	99.97

TABLE II
Handedness Formulae—257 Left-handed

Formula	Men		Women	
	No.	% of Total	No.	% of Total
LLL	38	25.00	34	32.38
LLR	41	26.97	41	39.05
LRR	71	46.71	27	25.71
LRL	2	1.31	3	2.85
Total	152	99.99	105	99.99

Table I permits a comparison of percentages for each formula within each group and between any two groups. Possibly the most impressive feature is the large number, more than half, who use the left hand wholly or partially for bimanual operations. This confirms Rife's remark, in the article already quoted in which he writes: "From extended observation I am sure that the majority of the human race use the broom and shovel left-handed, and bat right-handed."[9] This left-handed use of the broom is more pronounced for women and for the group of inferior men than for the group of superior men. The superior men give, however, a larger percentage for the RLL formula. This is the type that is right-handed unimanually and left-handed bimanually. Rife speaks of this as "the ideal type, from the standpoint of symmetrical bodily development."[10]

Table II summarizes the formulae of 257 left-handed individuals, without division into superior and inferior groups.

[9] *Op. cit.*, 475. [10] *Op. cit.*, 479.

The collection of cases for this table required considerable effort, a statement which applies with particular force to the data on left-handed girls and women, who were much more difficult to locate than the men and boys and are, possibly, on the whole, more frequently of a superior level.

A comparison of Table II with Table I is most instructive for the same three handedness types appear in reversed form, although in strikingly different proportions, especially for the LRR formula as compared with the RLL. The very great increase in the proportion of LRR individuals in Table II over the RLL individuals of Table I is certainly curious. If appeal is made to social pressure to explain the constitution of such opposed preferences in unimanual and bimanual activities, it must not be overlooked that such pressure would need to operate in opposite directions for the two contrasting groups (RLL and LRR). That the two formulae (RLL and LRR) probably point to two actually distinct types is suggested by facts to be reported in the next sections, for example that significant differences are found between them in the matter of the dominant thumb.

No calculations of significant percentage differences between Tables I and II was made since the left-handed group is a highly selected one. But such calculation was made for differences which appeared promising *within* Tables I and II. The calculations of these percentage differences were made for me by Mr. Harold Benjamin of Stanford University after consultation with Dr. Kelley.[11] They are tabulated in Tables III and IV. Table III shows the results for those differences which appeared promising in the tabulation for right-handed subjects.

It appears that the chances are more than 999 in 1000 that the percentage differences are significant for the relative frequency with which the formulae RRL and RLL occur in the case (1) of superior and inferior men, and (2) of superior men and superior women. That is to say a significantly greater proportion of inferior than of superior men are RRL in type and a significantly greater proportion of superior than of inferior men RLL in type; furthermore a significantly greater proportion of superior women are RRL in type as compared with superior men and a significantly greater proportion of superior men than of superior women are RLL in type.

[11] Mr. Benjamin's procedure was as follows: squared standard errors were computed for each of the categories under consideration (RRR, RRL, and RLL) using the formula $\sigma^2 p \frac{pq}{n}$; and then the standard error of the difference between any two categories was computed by the formula $\sigma p - p_1 = \sqrt{\sigma^2 p + \sigma^2 p_1}$. For example, σ^2 for RRR superior men $= \frac{(.4109)(1-.4109)}{421} = .0005749$ and σ^2 for RRR inferior men $= \frac{(.3562)(1-.3562)}{421} = .0005447$. $\sigma d = \sqrt{.000579 + .0005447} = .0335$. The Kelley-Wood table of the normal probability integral was used in estimating the chances that the differences were significant.

I shall attempt no explanation of this outcome. It may be the result of socially instilled habits; it may point to fundamental differences in nervous organization or functioning. I myself am inclined to believe the latter. Particularly with respect to sex differences in handedness I have been impressed by the consistency of the results throughout the whole investigation.

TABLE III
Chances that Percentage Differences of Table I are Significant

Formula	σd	Difference	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1000 that difference is significant
Between superior men and inferior men				
RRR	.0335	.0547	1.63283	949
RRL	.0336	.1141	3.39583	More than 999
RLL	.0236	.0736	3.11864	More than 999
Between superior men and superior women				
RRR	.0363	.0443	1.22039	889
RRL	.0377	.1666	4.55191	More than 999
RLL	.0220	.0867	3.94091	More than 999

TABLE IV
Chances that Percentage Differences of Table II are Significant

Formula	σd	Difference	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1000 that difference is significant
Between men and women				
LLL	.0576	.0738	1.28125	900
LLR	.0597	.1208	2.02345	978
LRR	.0588	.2100	3.57143	More than 999

It is rather curious that there is no evidence of significant handedness differences between superior and inferior women such as those found in the case of the two groups of men.

Within the left-handed group the question may be raised concerning the presence of significant differences in percentages between left-handed men and women. The calculations are given in Table IV.

As was true for the corresponding dextral types, so for the sinistral, a significantly greater proportion of men than of women belong to the LRR formula with a good chance that with a larger group a significantly greater proportion of women than of men would belong to the LLR type.

In Table II no division was made into superior and inferior subjects but the tabulation in Table V of sinistral tendencies for

21 male college professors and 15 subnormal boys is certainly impressive, since it is in harmony with the differences found in the dextral groups, particularly in the excess of LRR formulae for the superior group.

TABLE V

Formula	Male College Professors		Subnormals	
	No.	%	No.	%
LLL	7	33.3	2	13.3
LLR	3	14.2	10*	66.6
LRR	11	52.4	3*	20.0
Total	21	99.9	15	99.9

*In one case the batting habit was not stabilized.

The tabulation suggests the need of a resurvey of the left-handers in various institutions in order to determine the type of left-handedness most prevalent. It might also be worth while in the case of the feeble-minded to tabulate separately the manual habits of primary and of secondary aments.

(4) *Eyedness and handedness.* In the literature of handedness the question has frequently been raised concerning its connection with eyedness.

Recently Parson has reformulated in a bold unequivocal way the hypothesis that the preferred hand is determined by the sighting eye.[12] He states that "handedness results from an innate fault or limitation of binocular vision, on account of which all sighting or aiming operations are necessarily carried on unilaterally."[13] The necessity of monocular vision in sighting determines the dominant hand unless social pressure interferes to establish a crossed relationship. In spite of this determination of the preferred hand by the dominant eye Parson believes that handedness is *inherited*, probably according to Mendelian laws, left-handedness being a Mendelian recessive. Using an instrument of his own invention (the manuscope, recently renamed the manuoptoscope) Parson reports that about 30% are left-eyed; about 70% right-eyed; the same proportion would hold for right- and left-handedness, were it not for the operation of social pressure, that is to say nearly 30% of the population would be left-handed except for social interference.

Parson's hypothesis is supported by Mills who reports a very delicate technique for establishing eye-dominance.[14] He reports that "dextrality, aside from its recognized connection with a dominant right eye and a cyclophoric left eye, often is associated with more or less left hyperphoria and with the characteristic divergence of the left eye when a suitable test object is brought nearer than the convergence point. The reverse exists in sinistrality." Modifying conditions are described but the author concludes that a test for cyclophoria, or divergence inside of the convergence near point, may be considered as an almost infallible test for ocular dominance.

People fall into four groups, pure or crossed dextrals, and pure or crossed sinistrals. In the pure dextral there is right-handedness and a dominant right eye; in the pure sinistral, left-handedness and dominance of the left

[12]B. S. Parson, *Left-Handedness*, 1924, 1-185.

[13]*Op. cit.*, 51.

[14]Lloyd Mills, Eyedness and handedness, *Amer. J. Opth.*, 8, 1925, 1-9.

eye. In the crossed dextral there is right-handedness, but a dominant left eye; in the crossed sinistral left-handedness and a dominant right eye. The crossed dextrals, Mills explains, are "nearly always fundamentally left-handed but reversed by early training." The crossed sinistral is much the rarest combination and a "history often is obtained of injury to an arm, or of ocular disease or injury which explains the modification in ocular or manual dominance."

For five different series of 200 cases Mills reports the following percentages: "I found an average of pure dextro-ocularity in 76%; crossed in 13%; pure sinistro-ocularity in 9.3%; and crossed in 1.7%, which would place the total group of left-handedness at over 20%."[15]

Before stating the results of the present investigation that bear upon the relation of handedness to eyedness, I wish to state briefly points which seem to me inadequately developed by both Parson and Mills and which leave me unconvinced with respect to their conclusion that some 20-30% of the population are or should be left-handed.

(1) There is some inconsistency involved in the statements that the sighting line is a "direct instrument in the production of handedness" and that handedness is an inherited tendency. If handedness and eyedness are both dependent upon the same factor in heredity, an unambiguous test for eyedness might reveal native handedness, but it is difficult to see how eyedness can be said to produce handedness if the latter is already set by heredity.

(2) The relationship of both handedness and eyedness to a dominant cerebral hemisphere is left in doubt. One may, of course, confine oneself strictly to the *facts* revealed by the tests for eye-dominance and handedness and shelve the problem of hemispheral control. We are informed by anatomists that the macular portions of the two retinae, which are involved in sighting, are connected with both hemispheres of the brain. This may or may not be a fact of importance in the problem of relations between eye, hand, and brain. Parson states: "At least some of the maculary fibres of each eye are connected with both hemispheres."[16] He also remarks of his manuoptoscopic test "not the least important use to which it can be put is to enable us to find out *which cerebral hemisphere is the dominant one.*"[17]

(3) Both Parson and Mills state that their left-eyed subjects revealed, when questioned, sinistral tendencies so that there is strong evidence, they believe, that these left-eyed persons were naturally left-handed individuals whose native handedness had been tampered with. One would wish that these authors had been more explicit as to what tendencies they listed as sinistral. It is true that Parson describes many of his cases as shiftovers while Mills, in addition, writes, "some residual effect of an inherited tendency to sinistrality remained, such as batting or sweeping or wringing clothes with the left hand."[18] A glance at Table I (formula RRL) indicates that such an argument might prove too much since about 40% of men and over 50% of women sweep left-handed according to the usual notion. In addition, about 14% of men bat left-handed. One would scarcely dare to conclude that the left-handed outnumber the right-handed.

In fact, if Mills had questioned his right-eyed subjects concerning their manual habits as he questioned his left-eyed ones, he would have found many of them reporting sinistral tendencies such as left-handed sweeping and the like, and if such tendencies were considered significant he would have found his percent of crossed sinistrals greatly increased. From the standpoint of nervous instability, as we shall see, crossed dextrals and crossed sinistrals need intensive study. Genetic studies should also determine the relative time at which stabilization occurs for the sighting eye and for manual habits.

[15] *Op. cit.*, 5. [16] *Op. cit.*, 23. [17] *Op. cit.*, 87 (italics Parson's). [18] *Op. cit.*, 5.

Neither Parson nor Mills considers the possibility of a sex factor operating in eyedness but Rider, in an earlier article, presented some interesting data with reference to this matter.[19] Rider emphasized the significance of monocular vision in many occupations such as those of the mechanic arts in which accurate sighting is necessary and argued that unilateral winking (or voluntary closure of one eye alone) serves an important function in connection with such monocular vision. He investigated with great care the degree to which unilateral winking could be used as a test of comparative visual acuity, since, a priori, one would expect the poorer eye to be closed during monocular vision.

Name, eye, sex, manner of winking, acuity of vision without mydriatic and with, in cases in which its use was required, correcting lenses with resultant vision and various other facts which would tend to throw light upon the history of visual conditions were recorded for 2000 eye patients, seen mostly in private practice.

Among the equal-eyed, there were two classes, those winking equally and those winking unequally. The first class was numerically very much greater than the other. Habit, in the second class, operates to determine the closing of the left eye even when there is equality of vision in order that the right eye may work in harmony with the right hand. Rider apparently holds that in such case *right-handedness determines right-eyedness*, the reverse proposition to Parson's. He adds that this cause is more potent in men than in women because of differences in occupation. "While 80 per cent, or four-fifths, of the equal-eyed men who wink one eye better than the other unconsciously wink the left eye, the proportion among women is but 58 per cent." [20]

Three subdivisions occur among the unequal-eyed; those who wink equally, those who wink the better eye more easily, and those who wink the poor eye more easily. The last class is numerically much larger than the other two. If those who wink equally are eliminated, 86% are found to close the poorer eye. If cases are selected in which there is great difference in visual acuity the percent of those winking the poorer eye increases. We are pretty safe in saying that in the case of a man winking his right eye more easily than his left, his right eye is the poor one. Of unequal-eyed individuals who wink the better eye more easily a large proportion are myopes.

Rider's results are worthy of study, particularly with reference to the sex-difference he reports, to which reference will be made later.

(5) *Eyedness and handedness formulae.* In connection with the handedness formulae cited in the first part of the paper, I endeavored to obtain the sighting eye. In the questionary which I used the sighting eye was determined by the following simple experiment.[21]

Try the following experiment. Use a small finger-ring; hold it at arm's length, and encircle a small object a few yards away.

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[19]Wheelock Rider, Unilateral winking as a test of comparative visual acuity, *Trans. Opt. Soc.*, 8, 1898, 434-452.

[20]*Op. cit.*, 447.

[21]Cf. references cited by Clarence Quinan, Sinistrality in relation to high blood pressure and defects of speech, *Arch. Int. Med.*, 27, 1921, 257-261.

I. Focus on the object. Now close your right eye. Check what happens.

(A) No change
(B) Ring moves to right of object
(C) Ring moves to left of object

II. Refocus. Close your left eye. What happens?

(A) No change
(B) Ring moves to right of object
(C) Ring moves to left of object

Repeat this test until you are confident of the accuracy of your report.

In the above experiment, it should be noted, the right-eyed check I B and II A; the left-eyed, I A and II C; the either-eyed individual may check I B and II C.

The experiment is a simple one but there are numerous sources of error, for example:

(1) Inaccuracy in following directions, such as a slight movement of the head or arm after eye-closure so as to bring the object into focus.

(2) Inaccuracy in report, sometimes due to a tendency to report on the movement of the object rather than movement of the ring. In some cases there is a real confusion in discrimination of right and left. I have noted many of these in my work on subjects and one eminent psychologist penciled a note on the margin of his questionary to the effect that his first report had been inaccurate just because of such spatial confusion.

(3) Voluntary control of the sighting eye, due to expectation or other form of mental set. Sometimes, for instance, the use of right or left arm suggests the line of fixation. The most common error is, I believe, the report of I B and II C.

Because of the possibility of errors as listed above, my percentages are undoubtedly subject to some discount. In the work with groups less well trained than the professional psychologists results have, usually, been checked by an experimenter. Moreover, after the appearance of Parson's book, while the experiment was in progress, his instrument, the manuoptoscope, was obtained and used in connection with the ring test. In general, the results from the two were consistent although a small group was found who sighted *with one eye for far vision and with the other for near vision.* Moreover, Parson's test exhibits the same sources of error, except (2), that were found in the ring test. In extremely right- or left-eyed persons, however, it was possible for the experimenter to judge the sighting eye by the direction in which the tube of the manuoptoscope was pointed. Besides the ring and manuoptoscope test, unilateral winking and eye-opening was appealed to in doubtful cases.

In spite of the care used the records are far from accurate, particularly in the case of the left-handed, where only a small proportion of subjects could be personally tested. None the less, the attempt to bring together handedness formulae and sighting eye is of great importance since it indicates the complexity of the problem at which we are working.

Table VI summarizes the sighting eye for 585 right-handed men and 417 right-handed women, superior and inferior groups being indicated as before and Table VII for 131 left-handed men and 100 left-handed women. The numbers are smaller than in the preceding tables since in some cases, for example the Vineland Group, the sighting eye was not checked.

The probability of significant percentage differences in Tables VI and VII is summarized in Tables VIII and IX.

TABLE VI

Eyedness and Handedness—1002 Right-handed

Formula	Men						Women					
	Superior Group			Inferior Group			Superior Group			Inferior Group		
	Total No.	No. R-Eyed	% R-Eyed	Total No.	No. R-Eyed	% R-Eyed	Total No.	No. R-Eyed	% R-Eyed	Total No.	No. R-Eyed	% R-Eyed
RRR	138	111	80.4	119	83	69.7	110	77	70.0	46	27	58.7
RRL	120	88	73.33	118	74	62.7	151	92	60.9	79	53	67.1
RLL	61	48	78.70	29	23	79.3	24	16	66.7	7	5	71.4
Total	319	247	77.4	266	180	67.7	285	185	64.9	132	85	64.4

TABLE VII

Eyedness and Handedness—231 Left-handed

Formula	Men			Women		
	Total No.	No. L-Eyed	% L-Eyed	Total No.	No. L-Eyed	% L-Eyed
LLL	37	22	59.5	36	29	80.6
LLR	30	18	60.0	39	26	66.7
LRR	64	34	53.1	25	11	44.0
Total	131	74	56.5	100	66	66.0

TABLE VIII

Chances that Percentage Differences of Table VI are Significant

Formulae	σd	Diff.	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1000 that difference is significant
Between superior men and inferior men				
RRR	.0540	.1070	1.98148	976
RRL	.0601	.1063	1.76872	961
RLL	.0928	.0060	.06465	525
Total	.0370	.0970	2.62162	996
Between superior men and superior women				
RRR	.0607	.1040	1.71334	956
RRL	.0597	.1243	2.08208	981
RLL	.1221	.1200	.98290	837
Total	.0402	.1250	3.10945	More than 999

TABLE IX

Chances that Percentage Differences of Table VII are Significant

Formula	σd	Diff.	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1000 that difference is significant
Between men and women				
LLL	.1042	.2110	2.02495	978
LLR	.1170	.0670	.57265	716
LRR	.1172	.0910	.77645	781
Total	.0642	.0950	1.47975	930

The chances are more than 996 in 1000 that more right-handed men of the superior group (total of all formulae) are right-eyed than those of the inferior group, and that more superior men than superior women are right-eyed, with an excellent chance (981 in 1000) that RRL superior men are more apt to be right-eyed than women of the same formula.

It is, of course, possible that this sex difference is determined by the training men receive in sighting with the right eye in shooting, in various games, and in using tools such as the plane. Rider, who found a similar sex difference in his test on unilateral winking, was, as stated before, inclined to explain it in terms of the different pursuits of the two sexes.

The fact that the same difference is found when superior and inferior groups of right-handed men are compared should, however, incline us to caution in drawing such a conclusion. 'Crossed dextrals'—to use Mills' terms for the right-handed but left-eyed—are by certain investigators described as nervously unstable. The group of inferior men (psychopaths, defectives, and delinquents), might well be characterized by an undue amount of crossed-dextrality and in consequence by nervous instability. At least the possibility is worth further extensive investigation.

The summary for the left-handed shows some variation from that for the right-handed. As between left-handed men and women there is an excellent chance (978 in 1000) that more LLL women are left-eyed than LLL men. Again, the appeal to training is possible, more men than women being shifted to the right eye.

It is important here to note how definitely right-eyed the RLL group of men is, although the corresponding type (LRR) is not similarly left-eyed.

It is interesting to utilize the percentages given in Table VI and VII in determining the degree to which the present investigation agrees with former ones as to the amount of crossed dextrality and crossed sinistrality, that is, where dominant eye and hand are on opposite sides of the body.

The first summary relates to crossed dextrality, or the percentage of presumably right-handed persons who are left-eyed. Ballard in various series of tests estimates left-eyedness in right-handed children from 20-55%.[22] Quinan reports a great difference in the amount of left-eyedness depending upon the group tested. Among 572 right-handed inmates of a home for the aged he found about 24% left-eyed.[23] Two other observations of 100 each on machinists and on musicians gave, for 96 right-handed machinists, about 4% of left-eyedness and for 92 right-handed musicians about 26%.[24] This outcome is in harmony with Quinan's belief that crossed dextrality is symptomatic of a psychopathic constitution.

It is difficult to estimate from Parson's and Mills' figures the percentage of left-eyedness among the right-handed since their percentages are given in terms of the amount of right- and left-eyedness in total groups with only incidental observations on the manual habits except for out and out left-handers. They believe that all the left-eyed are natively left-handed, except in cases where ocular fault has developed. Parson's figures indicate that he found 180 left-eyed children (23%) among 784 presumably right-handed children.[25] From his total of 877 I have deducted not only 36 left-handed and 12 of impartial vision but also 45 who are listed as shiftovers or ambidexters. Mills reports 13% of crossed dextrals in a total of 1000 cases.[26] If the calculation is based on the 900 right-handed in this group

[22] P. B. Ballard, Sinistrality and speech, *J. Exper. Ped.*, 1, 1911-12, 298-310.

[23] *Op. cit.*, 257.

[24] C. Quinan, A study of sinistrality and muscle coördination in musicians, iron-workers, and others, *Arch. Neur. & Psychiat.*, 7, 1922, 352-360.

[25] *Op. cit.*, 106-113. [26] *Op. cit.*, 5.

of 1000, this percent is raised a trifle. Mills states, however, that about half of his crossed dextrals were actually ambidexters or shiftovers, and if the correction indicated is made, he actually found less than 8% who were crossed dextrals.

My own figures agree fairly well with those of other investigators who have sought to determine merely the 'sighting eye,' not necessarily eye dominance, at least in the records for the superior men, where I found about 25% of crossed dextrals. The groups of inferior men and of women had about 34% crossed dextrals, which is high, except as compared with some of Quinan's figures.

A second summary relates to crossed sinistrals, or left-handedness associated with right-eyedness. Ballard found that of 51 left-handed individuals 57% proved to be right-eyed.[27] Quinan found 50% of a group of 28 left-handers, right-eyed,[28] and about 21% of another group of 14.[29] Parson apparently found only 4 cases of left-handed children among his 608 right-eyed children.[30] It is not, however, clear that he questioned these children in order to discover how many had been forced to use the right hand in the same way that he questioned the children who proved to be left-eyed. Mills reports 17 cases, 15.4%, of left-handed individuals having a dominant right eye in a total of 110 left handed.[31] My estimate of crossed sinistrality (based on Table VII) is high, from 43.5% in men to about 34% in women.

Whatever conclusions may be drawn with reference to the origin of crossed dextrality or crossed sinistrality, the condition itself certainly deserves attention. Possibly the condition originates from the interference with native handedness by training or disease or from the development of ocular faults, or is even influenced by eye habits determined by social pressure. On the other hand, it is also possible to believe that crossed dextrality or sinistrality may be due to asymmetrical functioning of the nervous system. In any case Quinan's hypothesis that such crossed functioning is bound up with psychopathic tendencies is worth consideration. The general pattern of mental organization certainly appears to be influenced greatly by such asymmetry as having an opposite preference for hand and eye. The experimental results to be presented later show this.

One group of *Ss* has been very inadequately handled in the present report namely those who are either-eyed, or according to Parson's terms 'impartial in vision.' My own observations on this group led me to discriminate two sub-groups (1) those where there was a fluctuation in the sighting eye, and (2) those who were right-eyed for near vision and left-eyed for far vision or the reverse. My RLL handedness type gave me a number of the latter cases but I am not yet ready to draw any conclusions with reference to them.

(6) *The dominant thumb.* Lutz was, so far as I know, the first person to call attention to the fact that the same thumb is

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[27] *Op. cit.*, 304. [28] *Op. cit., Arch. Int. Med.*, 27, 1921, 259.
[29] *Op. cit., Arch. Neur. & Psychiat.*, 7, 1922, 359.
[30] *Op. cit.*, 107. [31] *Op. cit.*, 5.

for any given individual usually outside when the hands are clasped with the fingers alternating.[32]

On the basis of his returns from about 600 individuals Lutz concluded that the position of the thumb had apparently no relation to right or left-handedness and that no significant sexual dimorphism exists, but that the mode of clasping the hands is inherited. I have reported elsewhere my conclusions with reference to the dominant thumb.[33] I wish to repeat them here and to give a table covering a greater number of cases than that recorded in the article cited.

Contrary to Lutz's conclusions, I believe I have some evidence that the position of the thumb is related in some obscure way to right or left-handedness and that sex differences do

TABLE X
Thumb Out When Hands are Clasped

Formula	Men			Women		
	No.	No. R-Thumb Out	% R-Thumb Out	No.	No. R-Thumb Out	% R-Thumb Out
RRR	242	114	47.1	129	79	61.2
RRL	249	130	52.2	213	118	55.4
RLL	98	58	59.2	32	17	53.1
Total	589	302	51.3	374	214	57.2
LLL	34	10	29.4	34	16	47.1
LLR	41	19	46.3	39	19	48.7
LRR	68	24	35.3	26	11	42.3
Total	143	53	37.1	99	46	46.4

exist. His data concerning the inheritance of the trait, however, appear conclusive. My results are summarized in Table X. In Table XI calculations are given concerning the chance of various percentage differences in Table X being significant.

It will be seen from Table X that 51.3% of right-handed men and 37.1% of left-handed men have the right thumb up. The chances are more than 999 in 1,000 that this difference is significant, that is, that more right-handed then left-handed men clasp the hands, right thumb out. The approximately 50-50 proportion for right and left thumbs out in right-handed men must be interpreted in the light of a 37-63 proportion for left-handed men. Not only is there a significant difference for the total of right-handed and left-handed men but also a significant difference in the case of RLL and LRR men, where again the chances are 999 in 1,000 that the percentage difference is significant, that is to say that RLL men in a significantly greater

[32] F. E. Lutz, The inheritance of the manner of clasping the hands, *Amer. Natur.*, 42, 1908, 195.

[33] J. E. Downey, Further observations on the manner of clasping the hands, *Amer. Natur.*, 50, 1926, 387-391.

number of cases place the right thumb out than LRR men do, an outcome uniform to that found for the dominant eye. There is also an excellent chance, 982 out of a thousand, that the percentage difference for RRR and LLL men is significant.

Significant differences are not found in the records of the women, although possibly with larger groups they might occur. In the case of the total percentages for right-handed and left-handed women the chances in 1,000 that the difference is significant are 972.

TABLE XI
Chances that Percentage Differences of Table X are Significant

Formula	σd	Diff.	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1000 that difference is significant
Between right and left-handed men				
RRR and LLL	.0845	.1770	2.09467	982
RRL and LLR	.0841	.0590	.70155	759
RLL and LRR	.0763	.2390	3.13237	More than 999
Total rights and total lefts	.0453	.1420	3.13466	More than 999
Between right and left handed women				
RRR and LLL	.0958	.1410	1.47182	930
RRL and LLR	.0870	.0670	.77011	780
RLL and LRR	.1310	.1080	.82443	795
Total rights and total lefts	.0563	.1080	1.91829	972
Between men and women				
RRR	.0536	.1410	2.63059	995
RRL	.0465	.0320	.68817	754
RLL	.1012	.0610	.60277	727
Total	.0328	.0590	1.79878	964
LLL	.1159	.1770	1.52718	937
LLR	.1117	.0240	.21486	585
LRR	.1129	.0700	.62002	733
Total	.0644	.0930	1.44409	926

A comparison of the records for men and women indicates one significant difference, namely in the percentage of RRR men and women who put the right thumb out, more women than men being right-thumbed.

It is possible that the placing of the left thumb outside points to a slight degree of latent sinistrality since Lutz has presented evidence that the manner of clasping the hands is a heritable trait. The problem raised as to the significance of this trait is interesting particularly in the light of the sex differences that are suggested.

The experimental investigation to be reported later gave only slight indication that the dominant thumb deserved consideration. However my groups became so small when subdivided on the basis of (1) unimanua

and bimanual preference, (2) dominant eye, (3) dominant thumb, that I frequently ignored the latter basis of division and hence failed, perhaps, to reckon with it adequately.

Possibly a word should be added about the very small number of individuals, a fraction of one percent, who are either-thumbed, that is, place indifferently either thumb out when clasping the hands. Such individuals are usually highly unstable in other respects also, that is, they exhibit only a mild unimanual and bimanual preference and are apt to be either-eyed. They represent one type of ambidextral individual described previously.

(7) *The spade foot.* In my questionary investigation I asked for a checking of the spade foot. Rife says as regards the spade foot, "the fact is always overlooked that the foot used in spading is determined entirely by the bimanual dextrality,"[34] that is, individuals of RLL or RRL formulae will use the left foot on the spade, and LRR and LLR, the right foot. RRR individuals will use the right foot, and LLL, the left.

In checking over my questionary returns I found that for about 70% the spade hand was the same as the broom hand, that is, in the use of both instruments the same hand was placed toward the center. According to Rife we expect *complete agreement between the spade hand and the spade foot.* I found out of 226 respondents (men psychologists) some 44 cases in which the spade hand and spade foot were reported as opposed, that is, the right foot would be placed on the spade although the left hand was below the right in the use of this implement, or the reverse. Accordingly I sent out a second questionary to these 44 individuals, asking for a second report on the spade foot and hand. Thirty-seven of these questionaries were returned. I found that in 20 cases some error had been made in the earlier returns. The number of men placing the right hand down on the broom but the left hand down on the spade was increased over the first estimate. A number of others alternated right and left hand and foot on the spade.

Eleven men reported (two not confidently) that spade hand and foot were on opposite sides of the body. Their handedness formulae were as follows: 7 RRL, 1 RR_R, 2 RRR, and 1 RLL. In all of these cases the right spade foot was used with the left hand down on the spade, never the reversed situation. It is quite possible, as one respondent suggests, that spading for these individuals is really a unimanual performance, the right hand on the handle being competent to manipulate the implement. Shovelling would be a wholly different matter.

In general, Rife's contention that the spade foot follows the bimanual dextrality was strongly confirmed. LRR and LLR individuals place the right foot on the spade just as RRR in-

[34] *Op. cit.*, 479 f.

dividuals do. Curiously enough, 5 left-handed psychologists, LRR in type, reported in the first questionary that the left foot was their spade foot. All of them corrected this report in the second questionary. One RLL individual reports that the right is the spade foot.

Dextropedality has been very little investigated, certainly not to the extent it deserves. Ballard remarks that Lewis found 12 left-handed boys to be also left-footed. He himself reports "that 88 per cent. of the pure sinistrals were left-footed, and 86 per cent. of the dextro-sinistrals."[35] He adds: "It is not impossible that if acts of greater skill were required of the foot a still higher degree of correspondence between hand and foot would be revealed."

Rife makes the interesting suggestion that possibly the "faculties of springing and kicking are as independent of each other as uni- and bi-manual dextrality and possibly occur in somewhat the same ratio, that is, the great majority of people kick with the right foot while most of them spring with the left."[36]

Apart from my observations on the spade foot I have little information to give. In an experiment to be reported by Dr. R. S. Uhbrock, a record of tapping by both the right and left foot was obtained for 100 boys. Dr. Uhrbrock found 14 boys who tapped more rapidly with the left than with the right foot. One of these was a left-handed boy, formula LLR. The formulae of the other 13 have not been determined.[37]

(8) *Handedness formulae and degree of unidextrality.* Statistics have been given to show the relative frequency with which different forms of dextrality occur and the possibility of significant percentage differences in their frequency for certain formulae and for preferred thumb and eye when comparisons are instituted between the sexes and for different groups within the same sex. The next question that arises is this: are the handedness formulae related in any way to variations in degree of unidextrality?

In an attempt to answer this question, three tests were made of groups of *S*s belonging to different handedness types. The tests chosen were (a) pegging, (b) dynamometer, and (c) dexterity tests.

If the handedness formulae are significant one would anticipate finding RRR individuals more definitely unidextral than either RRL or RLL persons, although quite probably individuals of mild manual preference are most influenced by social pressure and so forced into the RRR group, a possibility which would blur results somewhat. Moreover the group of RRR subjects whose records are to be cited here contained some left-eyed and left-thumbed persons and was, therefore, not made up of pure dextrals. This defect in method was regrettable but unavoidable if the groups were to be kept of any size.

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[35]*Op. cit.*, 306. [36]*Op. cit.*, 480.

[37]For an interesting discussion of right- and left-sidedness in relation to rhythm see M. F. Meyer, *Psychology of the Other One*, 339 ff. Meyer concludes, "So, whenever there is any need or occasion for division of labor, the left leg does the heavy and the right leg the light (but skillful) work" (341).

The present series of tests were tried only on women. Comparable records should also be made with men. This is desirable especially for a comparison of RRR and RLL groups. So few women belong to this latter group that the results in this case are inconclusive.

In all tests the technique of procedure was carefully standardized. One great source of error was, however, evident throughout, namely, the failure of the *Ss* to work at their greatest possible speed at all times. Sometimes a desire to excel with one or the other hand appeared to influence the effort put forth. Records are given in terms of total performance for both right and left hands and in terms of a ratio obtained by dividing the right hand record by the left hand one.

(*a*) *Pegging Test.* For the pegging test the ordinary primary pegging board was used with wooden pegs. The board was 10 in. sq. and contained 100 shallow holes in 10 parallel rows. As many pegs as possible were set up in the holes during one minute's continuous work. A peg that fell after placement was counted. The right hand moved toward the right; the left hand toward the left. Three 1-min. tests were given to each hand in the following order: right, left, left, right, right, left. An average was obtained for the 3 trials with each hand and the ratio of performance for right and left hand calculated by dividing the average for the right hand by the average for the left hand. A long practice series was run for one individual of each handedness formula. This test will be reported by Miss Hazel Bowman.

The procedure in pegging was carefully standardized. The hand that was not pegging held the pegs and fed them to the other. To a certain extent the operation proved to be a bimanual one, rather than a unimanual one, a circumstance which reduces its value as a test of unidextrality, since skill with the left hand would speed up the pegging. Inequalities in the performances with the two hands are, however, evidenced by the fact that the coefficient of correlation for speed of the two hands in a group of 60 *Ss* was only +.43.

Table XII gives the results for the different formulae in terms of the mean speed for each hand and the mean ratio when the record for the right hand is divided by that for the left. There is some indication that the RRR group has the advantage in this test both with respect to the average total number of pegs placed and the ratios indicative of degree of unidextrality. The differences are small but consistent. The m.v.'s on the mean ratio indicate, however, considerable overlapping of the groups.

Reversals of handedness, that is, cases where the left hand made the better record (ratio under 1.00) occurred 3 times in the RRL_2 group but in no other group. There were also 3 instances of practical equivalence, ratio 1.05 or under, for the two hands in the RRL_2 group; one of equivalence for the RRR group; and 3 for the RLL group.

The long practice series carried out on one of each handedness type gave satisfactory evidence of the greater unidextrality of the RRR *S*. More than 100 trials with each hand were given to 4 *Ss* with the following results:

S		*Ulna-Plus Difference*[23]	*Average Ratio*
R	RRR—Right-Eyed—Left Thumb	Pronounced	1.34
D	RRL_2—Left-Eyed—Right Thumb	Medium	1.15
C	RRL_1—Either-Eyed—Left Thumb	Vanishing	1.06
B	RLL—Either-Eyed—Left Thumb	Vanishing	1.17

The average difference in number of pegs placed by the right and left hand was as follows: R, 19.54; D, 9.24; C, 3.26; B, 12.03.

[23]See Section 9.

TABLE XII

	Pegging						Dynamometer					
Formula	No. *Ss*	Mean 3 trials Right hand	Mean 3 trials Left hand	Excess Right hand	Mean ratio* R/L	M.V. on mean ratio	No. *Ss*	Mean 10 trials Right hand	Mean 10 trials Left hand	Excess Right hand	Mean ratio* R/L	M.V. on mean ratio
RRR	29	47.50	35.14	12.36	1.36	±.106	18	63.34	54.32	9.02	1.19	±.100
RRL_1	24	46.55	35.99	10.56	1.29	±.090	19	60.17	54.47	5.70	1.12	±.102
RRL_2	32	45.57	37.52	8.05	1.21	±.117	22	56.33	54.75	1.58	1.11	±.103
RLL	9†	44.60	37.50	7.10	1.18	±.099	9†	64.84	56.41	8.43	1.15	±.100

*Mean ratios were calculated from the ratios for individual *Ss*.
†Possibly one *S* in this group is wrongly classified since by all tests she appeared to be strongly right-handed. See footnote 43.

TABLE XIII

	Dexterity 1						Dexterity 2					
Formula	No. *Ss*	Mean no. taps Right hand	Mean no. taps Left hand	Excess Right hand	Mean ratio R/L	M.V. on mean ratio	No. *Ss*	Mean no. taps Right hand	Mean no taps Left hand	Excess Right hand	Mean ratio R/L	M.V. on mean ratio
RRR	17	354.0	290.0	64.0	1.22	±.075	17	523.6	405.7	117.9	1.30	±.093
RRL_1	14	330.7	288.6	32.1	1.15	±.084	14	514.1	413.2	100.9	1.24	±.076
RRL_2	14	336.8	297.1	39.7	1.14	±.062	14	533.6	435.0	97.7	1.23	±.123
RLL	8†	362.1	290.75	71.4	1.22	±.108	8†	501.4	413.2	88.2	1.22	±.135

*Mean ratios were calculated from the ratios for individual *Ss*.
†Possibly one *S* in this group is wrongly classified since by all tests she appeared to be strongly right-handed. See footnote 43.

(*b*) *Dynamometer Test.* For measuring the strength of hand grip with the two hands a dynamometer was used. Twenty readings under standardized conditions were taken, 10 for each hand, alternately, with proper rest intervals. The mean record was calculated for each hand and a ratio obtained to show the degree of superiority or inferiority of right over left hand (see Table XII).

Although the relative strength of the two hands has been considered by many investigators a very dubious test of handedness, the present classification into handedness groups indicates a superiority for the RRR type in the dynamometer test as well as in the pegging and dexterity tests. The mean ratios in the dynamometer test are, however, considerably lower than in the other tests.

Reversals (ratios of less than 1.00 indicating superiority of the left hand) are, moreover, more frequent in this test than in the two other tests. Nine occurred for 69 *Ss* distributed as follows: RRR 1 (18 *Ss*); RRL_1 3 (19 *Ss*); RRL_3 4 (22 *Ss*); RLL 1 (9 *Ss*).

Practical equivalence (ratio under 1.05) occurred once each for the RRR and the RRL_1 group and 4 times for the RRL_3 group.

(*c*) *Dexterity Tests.* For the dexterity test Scott's three-hole tapping apparatus was used. There was a 1-min. preliminary tapping test for each hand in which only one hole was used. This test is labelled 'Dexterity 1.' This was followed by 4 tests each 2 min. long, in which the 3 holes were utilized. The hands were tested in the following order; right, left, left, right. This test is labelled 'Dexterity 2.' The total number of contacts registered for each hand was used as the basis on which the ratios were calculated. The outcome is summarized in Table XIII.

As before, the results are, with one exception, (RLL, Dexterity 1) in favor of the RRR group for both Dexterity 1 and Dexterity 2. The *Ss* who are classified under the formula RRR and who are also right-eyed and right-thumbed give a ratio of 1.41 for Dexterity 2. It is possible that the eye habits affect the outcome of this test.

Reversals of handedness (ratio under 1.00) occurred twice in Dexterity 1, in both instances for *Ss* of the RRL formula. A practical equivalence of scores for the two hands (ratio under 1.05) occurred twice for RRL_3 *Ss* and once for RRL_1 *Ss*. There were no reversals of handedness in Dexterity 2 and a practical equivalence for only one RRL_3 *S*.

Conclusion. There is certainly some evidence that handedness formulae and degree of unidextrality are related but the experimental investigation needs to be supplemented by use of larger and more rigidly selected groups.

(9) *Structural differences and handedness types.* The question concerning the origin of dextrality types in native or acquired tendencies may possibly be settled by the discovery of significant structural differences between them.

(*a*) '*Ulna plus.*' The structural difference that has been suggested most frequently as a possible test of right or left handedness is that of relative bone measurements for the two halves of the body. Jones suggested measurements of the humerus and ulna, plus the distance to the middle knuckle of the little finger, and of the circumference of palm and wrist. He devised a special measuring instrument, the brachiometer, to get data on the humerus and the 'ulna plus.'

Beeley, who used the Jones' brachiometer in a test with young children, drew the following conclusion:[39] "The experimental application of the Jones' Brachiometer Test to 123 young children proves its unreliability for extensive use and also the invalidity of the theory upon which it is based, viz., that native handedness can be detected at any age by bone measurements."

Watson also reached a negative conclusion with reference to the value of measurements of right and left anatomical structures, such as width of right and left wrist, palm, length of forearm, etc.[40] He reports that measurements have been made upon several hundred children with specially devised instruments, but with results that show there is no significant difference in the right and left measurements. The average error of the measurement is reported to be greater than the observed difference.

Schwerz reports some interesting material on this point and cites bone measurements on adults from a number of investigators, who dealt each with a small *racial* group, a fact possibly of considerable significance.[41] Measurements were taken not only of the humerus, radius, and ulna but also of the femur and tibia. He deplores the inadequacy of the material and general lack of interest by anatomists in the comparison at stake, since if significant differences in bone-measurements should be discovered and associated with right- or left-handedness, a valuable method is at hand for determining racial differences in handedness, and, as he suggests, old graveyards could be made to yield profitable information. His general conclusions from study of the data at hand are as follows: there is a greater percentage of cases in which the left side measurements of radius and ulna exceed the right side measurements than is true for the measurement of the humerus, that is, the measurement of the humerus is more significant than the measurement of the ulna and radius. When all hand and arm measurements are in harmony, the presumption of handedness is greater than when the measures are inconsistent. The figures quoted by Schwerz show curious discrepancy of results for arm and leg. In the majority of cases the left femur and tibia were longer than the right tibia and femur. Schwerz remarks that the left leg is longer than the right and that our knowledge of the innervation of right and left side is very inadequate.

The problem of comparative bone-measurements is a very big one and obviously one to be handled by trained anatomists and anthropologists. My own material on this point is presented with some hesitation since I am thoroughly aware of its inadequacy. Yet the important point, as I see it, is the recognition of degrees of dextrality, which may be tied up with degrees of difference in bone-measurements. Massed records may easily obscure the point at issue. If possible, groups that show a big difference in bone-measurements on either side should be compared for handedness habits with groups that show a vanishing difference. That it is possible to identify such groups, at least among adults, I am convinced from my experience. In many cases a pronounced difference in ulna measurements, quite outside of range of error in measuring, is evident.

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[39]Quoted by Parson, *op. cit.*, 77.

[40]*Op. cit.*, 319.

[41]F. Schwerz, Die Rechtshandigkeit des Menschen, *Arch. f. Rassen- u. Gesellschbiol.* 11, 1914, 299-314. See also authorities quoted by Clarence Quinan, *Arch. Int. Med.*, 27, 1911, 256-261.

As part of the routine procedure in connection with getting the material reported in Section 8 of this paper three measurements each of the 'ulna plus' were obtained for right and left arms by means of Jones' brachiometer and an average taken. The instrument is not a delicately contrived one and, in addition, errors in manipulation are pronounced, so that inaccuracies in measurements are numerous, a vital matter when dealing with what is at best only a slight difference in lengths. One would like to see such measurements handled by X-raying of bones.

Altogether we have measures of the 'ulna plus' for 152 women, some of whom had been measured in a previous study. The median difference between the two arms was found to be about ⅛ in. A distribution of the measurements reported by Jones for women over 16 years of age gave the same median measurement.[42] The mean of our measurements was slightly less than 1/7 in. with a mean variation of about 1/12 in.

My assistant, Miss Bowman, and myself measured independently the same group of 26 college girls, taking three measurements each for right and left 'ulna plus.' We found that the mean difference in our 52 measurements was slightly less than 1/12 in., and that our mean difference in calculation of the 'ulna plus difference' was about the same, namely, 1/12 in.

TABLE XIV

Formula	Ulna Plus Difference			
	Vanishing		Pronounced	
	No.	%	No.	%
RRR	3	13.6	11	42.3
RRL	13	59.0	14	53.8
RLL	6	27.3	1	3.8
Total	22	99.9	26	99.9

From study of these measurements it appeared that if we were to select individuals who have a difference of approximately ¼ in. or more between the right and left 'ulna plus' and those who have a difference of 1/16 in. or less we would have groups whose difference in at least one bone-measurement would lie outside the range of error. Roughly, these groups correspond to the highest and lowest quartile as obtained in the distribution of the 152 girls.

Selection was accordingly made of all women with a difference between right and left 'ulna plus' of ¼ in. or more, and of 1/16 in. or less. Of this number I found that I had handedness formulae for some 48—for 22 who gave a vanishing difference between the two ulnas, and for 26 who gave a pronounced difference in ulna-lengths. I give above a tabulation of the handedness formulae.

The results of the tabulation seems to me highly promising, particularly for the RLL group. I discovered from study of my records that I had measurements of the 'ulna plus' of only 7 girls classified as RLL, and here I found six of them with a

[42] W. F. Jones, A study of handedness, *Univ. of South Dakota Pub.*, 1918, 1-80.

vanishing difference in length between the right and left ulna![43]

The contrast between these two groups is also maintained in the records taken for pegging, dynamometer, and dexterity tests, with one exception, namely Dexterity 2, where I believe the dominant eye complicates results.

To repeat, the results are distinctly promising. The experiment should be carried further. Bone measurements other than those of the 'ulna plus' should be used and an effort should be made to get a large group of RLL individuals. To obtain such, a group men subjects should be utilized.

TABLE XV

Test	Ulna Plus Difference			
	Vanishing		Pronounced	
	No.	Mean Ratio	No.	Mean Ratio
Pegging	23	1.25	22	1.33
Dynamometer	13	1.08	15	1.17
Dexterity 1	13	1.15	11	1.22
Dexterity 2	13	1.29	11*	1.27

*4 are left-eyed.

Skepticism concerning the value of bone-measurements as a test of native handedness has also been stimulated by reported discrepancies. Right-handed individuals may, for example, show an excess on the left side and left-handed individuals on the right. In all reports of this type the children measured are classified merely as right- or left-handed with no attention given to the type of dextrality. It is, therefore, of interest to canvas our material and to study discrepancies in the light of other records. It should, however, be stated that a summation of measures for hand and arm would be more satisfactory material than our single measure, namely, length of the 'ulna plus.'

Of 141 presumably right-handed women we found 14 (9.9%) with the left 'ulna plus' longer than the right. On 9 of these 14 cases we had data obtained in the present study. *Seven* of the 9 were classified under the group of 'Vanishing Difference' for right and left ulna; in every case the difference was less than 1/16 in. and so within the range of error. Five of the 7 were either left-eyed or either-eyed; two belonged to the formula RLL. There was only one RRR and right-eyed individual in the group which evidently is not a strongly right-handed one. Pegging and dynamometer ratios were also usually low for members of it.

Two of the 9 discrepant cases gave a pronounced difference in favor of the measurement of the left side. It amounted to ¼ in. for one girl. This *S* is, however, a cripple on the right side due to infantile paralysis at four years of age, and should not be cited adversely. It is, however, interesting to note that she is extremely right-handed in spite of her handicap. The second case, with about ⅛ in. difference in favor of the left side, is probably a naturally left-handed individual. Her pegging ratio was .89 (in favor of the left hand); her dynamometer ratio .96 (in favor of the left);

[43] I suspect the one individual of the RLL group who has a large difference between right and left ulna is wrongly classified as to handedness formula. The results of pegging, dynamometer, and dexterity tests suggest a strongly right-handed person. I have had no chance to recheck her reports on her batting and broom habits. See footnotes to Tables XII and XIII.

and her dexterity ratios 1.00 and 1.01. She reports certain left-handed tendencies but she appears to have been under no pressure in developing right hand writing. She is either-eyed and left-thumbed, RRL_1.

The above summary emphasizes the inadequacy of classifying individuals into two groups only if justice is to be done to the bone-measurement theory. The fact of importance is the degree of unidextrality.

Of 17 presumably left-handed women we found 3 (17.6%) with the right 'ulna plus' longer than the left. Of these three cases of discrepancy among left-handed individuals records were available for one only. This individual, who appears to be strongly left-handed but has been forced to use the right hand, writes a rapid and beautiful right-hand script. She stutters very badly, a fact usually attributed to the fact that she is a shiftover. Her formula is LLR, left eye, right thumb. This case deserves further study.

WHORL ARCH LOOP

PLATE I. Finger Print Patterns

(*b*) *Finger Prints.* As a second attempt to determine whether possibly structural difference are found between right and left-handed or between handedness types I made use of finger-print material, availing myself of Bonnevie's tabular summaries of the patterns characteristic of each of the 10 fingers.[44]

Since, as Bonnevie has shown, characteristic differences are found in right and left hands, it occurred to me to ask whether any of these differences would be reversed in a left-handed group. Also it seemed possible that finger-print material might be used to determine whether contrasting types such as LLL and LRR, or RRR and RLL would exhibit differences with respect to degree of symmetry or asymmetry of pattern.

Bonnevie's table for percentages of total frequency of patterns and for frequencies of each pattern for each individual finger is taken from a total of 245,180 fingers. The sexes are treated together and the right and left-handed included without distinction. The classification into patterns is the generally recognized one of whorls, radial and ulnar loops, and arches. (See Plate I.) As regards the general distribution of finger patterns Bonnevie remarks that they are very unlike with regard to their numerical appearance within the whole material. The whorls appear in a percentage of 25.65, loops in a percentage of 66.95, arches in a percentage of 7.4. With reference to a comparison of the right and left hands Bonnevie remarks, "the distribution of pattern-types between right and left hands also proves to be peculiar, the whorls being considerably more numerous on right hands

[44]Kristine Bonnevie, Main results of a statistical investigation of finger-prints from 24,518 individuals, *Eug., Genetics & Family*, I, 1923 200 ff.; Studies on papillary patterns of human fingers, *J. Genetics*, 15, 1924, 1-109.

(29.38 per cent. against 21.92 per cent. on left hands), while arches (6.92 per cent. to 7.88 per cent.) and especially also loops (63.70 per cent. to 70.20 per cent.) are more richly represented on left hands. This difference is, within the loops, due to ulnar loops only (57.76 per cent. to 64.52 per cent.) while the occurrence of radial loops (5.94 per cent. to 5.68 per cent.) is practically the same on both sides."[45]

The paragraph quoted above refers to percentage of pattern-types for all fingers taken together. Some consideration should, however, be given to a comparison of the separate fingers. Comparing corresponding fingers on right and left hands, Bonnevie remarks the high percentage of whorls for digits I and IV, especially upon right hands. Loops and arches yield a higher percentage on left hands. Digit II gives an approximate statistical

TABLE XVI
Finger-Print Patterns
96 Left-handers
(Table prepared by Richard S. Uhrbrock)

		Percent of Fingers				
Fingers	Hands	Whorls	Loops			Arches
			Rad.	Uln.	Total	
All	Both	26.67	5.63	62.81	68.44	4.89
	R	30.00	6.04	59.58	65.62	4.38
	L	23.33	5.21	66.04	71.25	5.42
I	Both	36.46	0	60.94	60.94	2.60
	R	40.62	0	56.25	56.25	3.13
	L	32.29	0	65.63	65.63	2.08
II	Both	25.00	23.44	38.02	61.46	13.54
	R	25.00	26.04	36.46	62.50	12.50
	L	25.00	20.83	39.58	60.41	14.58
III	Both	15.10	2.61	77.08	79.69	5.21
	R	17.71	1.04	77.08	78.12	4.16
	L	12.50	4.17	77.08	81.25	6.25
IV	Both	45.83	1.04	52.09	53.13	1.04
	R	54.17	1.04	44.79	45.83	0
	L	37.50	1.04	59.38	60.42	2.08
V	Both	10.94	1.04	85.94	86.98	2.08
	R	12.50	2.08	83.34	85.42	2.08
	L	9.38	0	88.54	88.54	2.08

correspondence between right and left hands for all patterns, although radial loops and arches are slightly more numerous on the right hand. Digit III shows nothing of importance so far as comparison of the two hands is concerned. Digit V gives a very considerably surplus of whorls upon right hands as compared with left. Also radial loops are unequally distributed, being more frequent on the left than on the right. The above summary refers to total percentages for the different papillary patterns.[46]

Table XVI gives a similar summary for 96 left-handed *Ss*. The table was prepared by Dr. R. S. Uhrbrock who was well trained in handling finger-print technique. It will be noticed that in general the above com-

[45] *Op. cit., Eug. Genetics & Family*, 1, 1923, 200.

[46] For discussion of another sort of bimanual distinction, see Harold Cummins and Charles Midlo, Palmar and plantar epidermal ridge configurations (dermatoglyphics) in European-Americans, *J. Amer. Phys. Anthrop.*, 9, 1926, 471-572.

parison between right and left digits held true, but in three instances, there were reversals of percentages on right and left fingers as follows: (a) Digit I: arches; (b) Digit II: arches; (c) Digit V: radial loops.

Besides these rather striking reversals some changes in percentage of patterns occurred that seem worth noting. Whorls give about the same percentages, as Bonnevie's tables show, for all fingers of both hands and there are no reversals for right and left hands but there is an increase of about 4% of whorls on left Digit I (thumb), and an increase in percentage of whorls for the sum total of both hands on Digit IV, with a slight decrease for Digit II.

Radial loops show one reversal with respect to right and left on Digit V and also a striking increase in the percentage on left Digit III.

Ulnar loops show an increase of approximately 6 or 7% for the sum total of both fingers on Digits II, and III and slight decrease on IV.

Arches gave a decreased proportion on the whole for all fingers. Two reversals of percentages for right and left occurred, namely on Digits I and II.

Table XVII gives the distribution of patterns upon the different fingers in percentage of the type. Bonnevie's corresponding percentages are given in parentheses for totals in order to show that there is, in general, close agreement in spite of the great difference in number of fingers utilized in the two investigations, that is, totals for right- and left-handed yield similar results but with a few differences which may or may not be significant.[47]

To specify, on Digit I, whorls, ulnar loops and arches give approximately the same percentages in the two investigations, but radial loops run slightly higher in Bonnevie's tabulation. On Digit II whorls are reduced in percentage and ulnar loops and arches are increased for the left-handed. On Digit III the percentage of arches is decreased. On Digit IV the percentage of whorls and radial loops is increased, and the percentages of ulnar loops and arches are decreased. On Digit V the percentage of radial loops is decreased and of arches increased.

In specific comparison of correspondences for right and left fingers, the following reversals of percentages occurred: (a) Digit I: arches; (b) Digit II: loops (total) and arches; (c) Digit V: radial loops.

In commenting upon her statistical material Bonnevie asks the reason for the great frequency of whorls upon Digits I and IV, as well as the striking surplus of their percentage upon the right hands, a feature which is visible upon all fingers but especially upon Digit I. She also calls attention to the strange difference between right and left hands with regard to radial loops on Digit V, their value on the left side being more than 18 times that on the right. My own tabulation shows a slight increase of percentage of whorls on the left hand as compared with the right and also in the case of Digit V, a reversal in the percentage of radial loops on right and left hands, which is most interesting in the light of Bonnevie's finding.

In general, it will be seen that there is sufficient evidence of shifts in relative percentages for the right and the left hand to warrant a further and more comprehensive investigation. It is necessary to have a much greater number of left-handed subjects (pathological cases being sifted out if possible), to separate the sexes (since differences in handedness occur), and to keep constant the dominant eye. Possibly, also, tabulations should be made keeping constant the dextrality type and even the

[47] *Op. cit., J. Genetics*, 15, 1924, 12.

TABLE XVII

Finger Print Patterns
96 Left-handers
(Table prepared by Richard S. Uhrbrock)

Pattern Type		All Fingers R	All Fingers L	Digits I R	Digits I L	Digits II R	Digits II L	Digits III R	Digits III L	Digits IV R	Digits IV L	Digits V R	Digits V L
Whorls	No.	144	112	39	31	24	24	17	12	52	36	12	8
	%	56.25	43.75	15.23	12.11	9.38	9.38	6.65	4.69	20.31	14.06	4.69	3.51
	*%	100		27.34†	(27.32)	18.76	(22.53)	11.33	(12.64)	34.37	(28.93)	8.20	(8.58)
Loops (Radial)	No.	29	25	0	0	25	20	1	4	1	1	2	0
	%	53.70	46.30	0.0	0.0	46.30	37.04	1.85	7.41	1.85	1.85	3.70	0.0
	*%	100		0.0	(1.15)	83.34	(82.57)	9.26	(7.96)	3.70	(2.70)	3.70	(5.62)
Loops (Ulnar)	No.	286	317	54	63	35	38	74	74	43	57	80	85
	%	47.43	52.57	8.96	10.45	5.80	6.30	12.27	12.27	7.18	9.45	13.27	14.10
	*%	100		19.41	(19.86)	12.10	(10.03)	24.54	(23.05)	16.58	(19.20)	27.37	(27.86)
Loops (Total)	No.	315	342	54	63	60	58	75	78	44	58	82	85
	%	47.95	52.05	8.22	9.59	9.13	8.83	11.42	11.87	6.70	8.83	12.48	12.93
	*%	100		17.81	(18.24)	17.96	(16.33)	23.29	(21.73)	15.53	(17.77)	25.51	(25.93)
Arches	No.	21	26	3	2	12	14	4	6	0	2	2	2
	%	44.68	55.32	6.38	4.26	25.53	29.78	8.51	12.76	0	4.26	4.26	4.26
	*%	100		10.64	(10.58)	55.31	(44.50)	21.27	(29.81)	4.26	(9.23)	8.53	(5.88)

*Percentages both right and left combined.
†Bonnevie's corresponding percentages are given in parentheses.

dominant thumb. If significant reversals of percentages should be obtained from the use of large and definitely selected groups the results would indicate that handedness is linked up with structural items in such a way as to warrant the conclusion that it has its basis in inherited rather than acquired characters.

One other approach will be illustrated in this study but merely as an example of the way in which finger-print material might be used. My groups are too small to cite in any conclusive way. It was thought that it might be possible to obtain an index of degree of symmetry for the right and left hands for different dextrality types. It seemed possible that the degree of symmetry in RLL and LRR cases might be greater than that found in RRR and LLL cases. Bonnevie has a table summarizing for 245,180 fingers the percentages of correspondence between fingers of the right and

TABLE XVIII

Percentage Correspondence Between Fingers of Right and Left Hands

Digits	% Bonnevie	Total 1890 Fingers	Right-Handed 830 Fingers	Left-Handed 1040 Fingers	RRR and LLL 420	RLL and LRR 680
I	74.05	76.71	77.10	75.96	71.42	76.47
II	51.28	55.55	54.21	56.73	57.14	55.88
III	73.83	75.13	72.28	76.92	69.04	70.58
IV	71.91	78.30	78.31	77.88	76.19	82.35
V	87.61	84.65	85.54	83.65	76.19	85.29

left-hands both for totals and for specific patterns.[48] I have attempted to make comparisons for my material for the totals for each finger. See Table XVIII. In the first column I give Bonnevie's total percentages and then in succession percentages (1) for my totals (1890 fingers), (2) for the right-handed (830 fingers), (3) for the left-handed (1040 fingers), (4) for combined RRR (Right Eyed) and LLL (Left Eyed) (420 fingers), (5) for combined RLL and LRR (680 fingers). Both sexes were used in getting the data for this table.

The results show that for Digits I, IV, and V there is a greater degree of correspondence for RLL and LRR, or symmetrical, types than for RRR and LLL, or unilateralized types, but that on Digit II, there is a slight reversal of percentage, and on Digit III no evidence of difference. As compared with Bonnevie's figures, Digit II shows a greater degree of correspondence than she obtained. On the whole, there is some evidence in confirmation of one's expectations as regards varying degree of symmetry in contrasting types.

One further attempt was made to compare contrasting types RRR, LLL with RLL and LRR. Ridge counts were obtained for all patterns on all 10 fingers of each member of the two groups. In order to use together loops and whorls, the double ridge count was made for the whorl and the sum divided by 2; arches were counted as zero. This is the method of summation suggested by Bonnevie.[49] A difference between the ridge count

[48] *Op. cit.*, *J. Genetics*, 15, 1924, 14.

[49] She says, "As a basis of classification has been used the method of ridge-counting, previously applied to loops only. This method has here been developed for application to other patterns in the way now to be described." "The quantitative value of any pattern is expressed in two figures based upon the number of ridges between each of the two tri-radii and the center of the pattern (the centre of a typical whorl, the core of a loop, the two cores of a double loop). If there exists only one tri-radius (loops) the figure of the other side is 0; if no tri-radius exists (simple arch) the figures of the pattern are 0—0" (*ibid.*, 47).

on corresponding right and left fingers was then obtained, the sum of the differences totaled for the two groups, and a mean obtained by dividing by the number of fingers in each group. The mean differences for the RRR and the LLL groups was 2.579, and for the RLL and LRR groups, 1.781. It will be noticed that this difference in symmetry is again in favor of the RLL and LRR group and again confirms one's expectations.

It occurred to me that it might be interesting to test out the possibility of the dominant thumb, or the thumb outside when the hands are clasped with the fingers interlacing, being related to finger-patterns. Accordingly I obtained the percentages of whorls and of loops and arches for the right thumbs of those individuals who placed the right thumb out when the hands are clasped, and also the percentages for the left thumbs in this same group. I obtained also the percentages of whorls and loops and arches for the left thumbs of those individuals who placed the left thumb out when clasping the hands and the percentages for the right thumbs of this left-thumbed group. For results see Table XIX.

TABLE XIX

	Right Thumb out (126 cases)						Left Thumb out (127 cases)					
	Whorls		Loops		Arches		Whorls		Loops		Arches	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
R Thumb	56	44.4	69	54.7	1	00.8	48	37.8	77	60.6	2	1.6
L Thumb	39	30.9	86	68.2	1	00.8	38	29.9	82	64.6	7	5.5

I should be unwilling to attempt to draw any conclusions from this tabulation since the groups are small, since more than a third of the *Ss* were left-handed, and since no differentiation was made on the basis of sex. The figures suggest, however, the sort of observation that should be made on more satisfactory material in the hopes of finding significant differences. For example, Bonnevie finds 41.60% of whorls on right thumbs (245,180 fingers), whereas the above tabulation gives 44.4%, the right thumb being outside when the fingers are clasped, and 37.8% when the left thumb is out, a shift which may be worth following up. Nothing similar appears in the percentages for right and left thumbs for left-thumbed people. Bonnevie gives 28.42% of whorls on the left thumb. The present investigation gives 29.9% when the left thumb is outside and 30.9% when the right thumb is outside. In the case of the left-handed one gets an increased percentage of whorls on the left thumb as was previously mentioned. The fact that so many of the group used in the above tabulation were left-handed makes an interpretation of results difficult.

The tabulation suggests that a study should be made of the percentages of arches on right and left thumbs of right- and left-thumbed individuals respectively. The present tabulation suggests an increase in percentage of arches on the left thumb of left-thumbed people.

(10) *Handedness types and intelligence.* The foregoing discussions have given us some reason to believe that handedness types, *i.e.* variations in unidextrality called types for convenience, do exist and have possibly a structural foundation. The next question concerns their further significance. Is the degree of unilaterality of function indicative only of greater motor speed and skill on the part of the more highly unilateralized individual or has it far-reaching implications with reference to the general

constitution of the individual? It is quite conceivable that the latter is the case. Symmetry or asymmetry of structure and function may be related to certain as yet obscure but fundamental factors in development.

It is possible that we must look to such investigations as Newman's on the biology and physiology of twinning for an ultimate explanation of handedness.[50] Such studies imply that bilaterality of structure is a form of twinning. Newman suggests that the prevalent belief that one twin is physically superior to the other may have some foundation in fact. As a consequence of the twinning relation one twin has a tendency to gain a physiological ascendency over the other. Unilaterality of function might arise similarly in man. The preferred half is the 'superior twin.' One would anticipate, however, all degrees of difference between the two body-halves, just as one finds all degrees of difference between completely developed twins.

Since the goal of the biological processes appears to be the development of a bilaterally symmetrical individual, it is natural to conceive *a priori* the probability of the RLL or LRR individual being the physiological type in which the developmental process has been carried through most adequately. RRR and LLL individuals suggest a high degree of lateralization of function which if too extreme might result in psychic imbalance.[51] RRL and LLR suggest a mixed functioning which might be tied up with nervous instability.

It is quite possible that the degree of unilateralization of function may exert a supreme influence not only on the patterning of behavior but also in the patterning of psychic reactions.

At the present stage of the investigation we are limited largely to speculation with some curious differences of opinion concerning the relative value of ambidextrality and unidextrality. Many believe that the ambidextrous are less mentally gifted than the unidextrous, and that lateralization of brain function is essential to higher mental development. On the other hand, the advocates of ambidextral training believe that increased cerebral capacity accompanies training of the two hands. Kerr described dextrality as "the specialized aptitude associated with the psychical phenomena of speech and reasoning which is so marked in the left brain," and recognized three classes as follows. (1) The highest development of both hands in true ambidexterity, accompanied by high intellectual and expressive attainments. (2) One hand well developed in function, the other mainly accessory, the condition of the immense majority with whom right- or left-handedness is an accident and not expected to affect mentality. (3) Defective functioning of both hands, really an ambisinistrality which includes many of the so-called left-handed.[52] Many individuals who have not developed properly mentally and who show many degenerative stigmata such as speech defects are said to be 'ambisinistral.' Ioteyko urges attainment of ambidextrality not only to establish a complete equality of the two sides of the body but to correct such inequalities as grow out of a predominately unilateral training, since the asymmetry extends to the psychic centers.[53]

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[50]H. H. Newman, *The Biology of Twins*, 1917, 1-179; *The Physiology of Twinning*, 1923. 1-219.

[51]In this connection A. Gesell's Hemihypertrophy and mental defect (*Arch. Neur. & Psychiat.*, 6, 1921, 400-421) is most interesting. Gesell says, "the facts clearly indicate that hemihypertrophy should be added to the list of developmental anomalies which bear some lawful relation to the incidence of mental deficiency."

[52]J. Kerr, Left-handedness and mirrored writing, *School Hygiene*, 11, 1920, 39-44, 74-82

[53]J. Ioteyko, L'usage de la main gauche chez les mutilés, *Rev. scient.*, 1916, 494-499.

Whatever difference of opinion may exist about ambidextrality it is a common belief that left-handedness is a sign of mental deviation of some sort. This belief is based largely on the very high percentage of left-handers found among the delinquent and feebleminded. The following figures are taken from Gordon's extensive and careful investigation:[54]

% in ordinary schools	7.3	(3,298 children from 4-14 years).
% in mental defective schools	18.2	(4,620 children from 8-16 years).

The percentage of left handedness among idiots and epileptics is estimated to be from 16-30%.

Although in ordinary schools the percentage of left-handed boys is greater than the percentage of left-handed girls, Gordon found that in schools for the mental defectives the percentage of left-handed girls was about 25% higher than that of boys.

Gordon also reports that while among normal children the left-handed are frequently the most efficient and capable, among mental defectives it is exactly the reverse, left-handedness being often associated with marked deficiency. He adds, "spontaneous change of left-handedness to right-handedness in writing among mental defectives is often found to be associated with progress in school work and in intelligence."

On the basis of the above facts Gordon framed the following tentative hypothesis. "There seems therefore to be good reason to believe that there are certainly two distinct types of left-handedness. (1) Those who are naturally left-handed, *i.e.* those who have a dominant right hemisphere. (2) Those who are naturally right-handed, but who have been driven to the use of the left hand in some of their activities, owing to some defect of the nervous or muscular system."

The left-handedness prevalent among mental defectives is according to Gordon's hypothesis largely of the pathological type. The left hemisphere is affected and this disturbance not only interferes with the proper functioning of the right hand but also with the functioning of many of the higher intellectual centers.

Gordon's percentages are based upon unimanual activities. The present investigation has insisted upon the need of study of the bimanual performances also. It has, however, presented evidence that indicates that the type of left-handedness most prevalent among feeble-minded males (LLR) is not the same as that found among American psychologists (LRR). An extensive survey should be made of delinquent and feeble-minded groups to determine whether or not the indications reported in the present paper are confirmed. If so, the facts should then be reviewed in the light of Gordon's hypothesis.

Some experimental work has been done on the mentality of the left-handed. Ballard who was particularly interested in the lingual ability of dextro-sinistrals or those sinistral children who had been forced to use the right hand in writing, found that the results of his researches falsified his expectations that they would be below normal. He cites his results as indicating "that sinistrals, as a class, are in no way deficient in their general power of verbal expression; for although the pure sinistrals are better than the dextro-sinistrals, *both are better than the 'dextrals'*".[55] Because of the small numbers involved Ballard does not press this inference, but he does conclude that "the phenomena of dextrality (in its broader sense) are manifested in the motor rather than in sensory bilateral asymmetry."

Gordon in an investigation of 1,019 dependent children concluded that the left-handed were not at all inferior to the right-handed in mental level.[56]

[54]Hugh Gordon, Left-handedness and mirror writing, especially among defective children, *Brain*, 43, 1920, 312-368.

[55]*Op. cit.*, 30, italics mine.

[56]Kate Gordon, Some notes on the mental status of the left-handed, *J. Delinq.*, 8, 1923, 154-157.

I have Thorndike scores on 43 left-handed college students. Nearly 25% of this group have a score in the 80's and 90's, an exceedingly fine showing. It may be of interest to note that only one of this group is LLR in type.

The problem of the relation of intelligence and handedness involves, however, much more than the question of the mental deviation of the left-handed. From my point of view it centers in the determination of the significance of the different dextrality types. One may group together the formulae RRR and LLL, RLL and LRR, RRL and LLR and ask whether there are any facts that seem to point to mental differences between these

TABLE XX

Handedness and Intelligence
115 Men
(Mean Thorndike 66.32—1919-25 Series)

Formula	Total No.	Above Mean No.	Above Mean %	Below Mean No.	Below Mean %
RRR & LLL	37	26	70.27	11	29.73
RLL & LRR	35	22	62.86	13	37.14
RRL & LLR	43	22	51.16	21	48.84
Total	115	70	60.86	45	39.13

TABLE XXI

Handedness and Intelligence
201 Women
(Mean Thorndike 62.51—1919-25 Series)

Formula	Total No.	Above Mean No.	Above Mean %	Below Mean No.	Below Mean %
RRR & LLL	69	34	49.27	35	50.72
RLL & LRR	20	13	65.00	7	35.00
RRL & LLR	112	68	60.71	44	39.28
Total	201	115	57.21	86	42.78

three groups. The problem is complicated by the undoubted existence of sex differences in handedness. The material to be presented must be studied in the light of such differences.

The statistical outcome of Section 3 indicated that superior and inferior groups of men give significant differences as regards the percentages falling under certain handedness types. Significant sex differences for certain formulae were also indicated. In order to carry this investigation further a tabulation was made of the handedness types of 201 college women and 115 college men in connection with their Thorndike scores. They were entered as above or below the mean Thorndike as calculated separately for 579 Freshmen women for the series 1919—1925, and for 571 men.

Tables XX and XXI give the tabulation. I refrain from drawing any conclusions from them since the figures show that

some unknown factor of selection operated to give me more handedness records from individuals above the mean for intelligence than below it. The tables do suggest, however, the possibility of certain significant percentage differences existing and indicate the same trend of evidence as that exhibited in Section 3, namely, the existence of some relationship in males between superiority and either high unilaterality of function (RRR-LLL) or symmetry of function (RLL-LRR), a superiority not evident in the RRL (LLR) or mixed group.[57]

The case with the women is different, except for the RLL (LRR) formula where the results are comparable to those obtained from the men. The point of difference is this: that the RRL (LLR) women make a better showing on an intelligence test than the RRR (LLL) type. The interpretation of the sex difference revealed in the above tables remains obscure. No doubt the problem is one that the physiologist rather than the psychologist must solve.

To check further the relationship existing in the women between asymmetry and intelligence the mean Thorndike was calculated for two contrasting formulae, keeping the eye constant, with the following results:

RRR–Right-eyed = 67.57 (29 cases).

RRL_3–Left-eyed = 74.20 (22 cases).

Calculation was made for the same two formulae, keeping the thumb also constant, with the following results:

RRR–Right-eyed–Right-thumbed = 65.30 ± 9.39 (16 cases).

RRL_3–Left-eyed–Left-thumbed = 76.47 ± 11.81 (10 cases).

One would not, of course, care to draw conclusions from such small groups but since the trend of evidence is all in the same direction it remains for future workers to investigate further the possible relationship between intelligence, probably of the verbal and abstract variety, and asymmetry or mixed type of handedness.

It has long been my belief that differences in handedness are bound up with perceptual and ideational predispositions and that sex differences are also found within the same field. The particular correlation in mind is that of a possible relationship existing between extreme unidextrality and visual or spatial predispositions, and between mixed, or asymmetrical functioning and linguistic interests.[58] With reference to the latter point

[57] In this connection I may cite the handedness formulae of 18 psychologists who have been presidents of the American Psychological Association: RRR, 10; RLL or LRR, 5; RRL, 2 (one is possibly RLL); RRR or RLL, 1. The group is highly lateralised in function or ambidextral in the sense of an opposed unimanual and bimanual preference.

[58] I may say that while the author was working on the above hypothesis, a letter was received from Professor Yerkes which contained the following paragraph: "In connection with your studies of structural and functional asymmetry, I wonder if you have thought of trying to measure, for purposes of correlation, linguistic ability and related memory functions. I have long suspected that facility in learning a language is intimately related to asymmetry of motor functions. There certainly is amazing differences among individuals in linguistic ability as well as in the traits which you have been studying by means of the Will-Temperament Tests and your tests of asymmetry."

some further data were obtained. Four vocabulary tests (namely, Starch 1 and 2, Kirkpatrick, and Thurstone) were given to 100 girls. Percentile distributions of scores were made for each vocabulary test and then an average percentile rank obtained for each *S*. An average was then obtained for each of three handedness groups as follows:[59]

RRR-Right-eyed = 48.70 (17 cases),
RRL-Right-eyed = 51.33 (28 cases),
RRL-Left-eyed = 59.14 (24 cases).

It will be noticed that the last group in which there is asymmetry between hand and eye as well as a mixed dextrality type is distinctly the superior group. In the next section it will be shown that this combination of right-handedness and left-eyedness is a great *disadvantage in an orientation test.*

TABLE XXII

High in Vocabulary Test

S	Thorndike	Formula		Rank in Vocabulary	Rank in Hand Test
S	99.9	RRL_1	L-E*	1	34
T	79.3	RRL_2	L-E	2	69.5
Sh	82.5	RRL_3	L-E	3	69.5
Co	111.0	RRL_1	R-E	4	24.5
Cr	71.6	RLL	L-E	5	45
U	†	RRL_2	E-E	6	73
Sn	77.9	RRL_1	R-E	7	60
H	91.3	RRR	R-E	8	60

Low in Vocabulary Test

S	Thorndike	Formula		Rank in Vocabulary	Rank in Hand Test
Hi	44.6.8	RRR	L-E	70	34
Ho	71.8	RRR	R-E	71	49
R	63.7	RRL_3	R-E	72	60
M	31.6	RRL_1	L-E	73	15
F	63.5	RRR	R-E	74	66
V	44.8	RRL_1	R-E	75	15
Va	50.8	RRL_2	L-E	76	30.5
Ch	59.7	RRL_2	R-E	77	39.5

*L-E = Left-eyed; R-E = Right-eyed; E-E = Either-eyed.
†No record, but undoubtedly of superior intelligence.

Some further evidence was obtained through giving to the same group of 100 girls the psychological examination published by the American Council on Education and obtaining a percentile distribution for such tests as those entitled 'Artificial language,' 'Reading' and 'Opposites,' etc. The sub-groups are small but possibly the following facts are significant: In the test on 'Opposites' only 1 RRR-Right-eyed individual appears in the highest 10%, while there are 7 RRL-Left-eyed individuals. In the test on 'Artificial Language' there is only 1 RRR-Right-eyed individual in the highest 10%, and 6 RRL-Left-eyed. In the reading test there are 2 RRR-Right-eyed in the highest decile, and 5 RRL-Left-eyed.

It would be most interesting if one had for comparison with the records above, the records of the same groups of girls on a mechanical aptitude test, but I do not have such material.

But in contrast to the result on the verbal tests I may cite those obtained from Thurstone's 'Hand test' which is described as a spatial rela-

[59]The group RLL was too small to use.

tions test and consists of 49 pictures of the right and the left hand in different positions. "The problem to be solved is whether the figure is that of the right or left hand." The rank correlation with the results of the four vocabulary tests is practically zero, actually slightly negative (−.12) for 77 college girls. A tabulation of the rank standing in the two tests for the 8 *Ss* who were respectively highest and lowest in the vocabulary tests is instructive (see Table XXII), for it seems to indicate that those who rank highest in the vocabulary tests (all have high Thorndike scores) are at some special disadvantage in the 'Thurstone Hand-test' although those who rank low in the former test are not necessarily high in the latter.

The special ability tapped by the 'Thurstone Hand-test' is not clear to me. I could find no evidence that success in it was dependent upon degree of unidextrality. It can be manipulated in two different ways, visually or kinesthetically, but does not, apparently, lend itself to verbal or symbolic manipulation. In this latter respect it differs from the 'Orientation Test,' a second spatial relationship test which will be reported in the next section. A rank correlation for 31 *Ss* for the hand and orientation test gave a coefficient of +.09—practically a zero relationship.

Formula	% Reporting Confusion	
	R-eyed	L-eyed.
RRR	28.9	51.9
RRL	35.5	58.3
RLL	23.3	50.0

(11) *Orientation and eyedness.* In tabulating the results of my questionary investigation of the handedness of psychologists relative to the percentage of each dextrality type reporting a tendency to confuse right and left, I discovered a significant difference in the amount of spatial confusion reported by right-handed respondents who were right-eyed and those who were left-eyed.

I quote below, from my article "Distinguishing Right from Left,"[60] a summary of the returns from 257 questionaries received from right-handed individuals—67 checked questionaries from students were added to the number sent in by psychologists.[61]

Right-handed respondents who were left-eyed reported a greater amount of confusion than those who were right-eyed. LLL respondents, on the other hand, reported more spatial confusion when right-eyed than when left-eyed, a result in harmony with that obtained from right-handed and left-eyed respondents. It appeared from the questionary returns that a lack of agreement between hand and eye served to mediate confusion in distinguishing between right and left.

This discovery appeared to me so significant that I endeavored to confirm it by the use of experimental material. In order to do this it was necessary to devise a group test for orientation.

[60] J. E. Downey, *Ped. Sem.*, 33, 1926, 153-166.

[61] For totals on which this summary is based and application of the formula to determine significant differences see *ibid.*, 164.

Adopting and extending a suggestion made by Terman and Chamberlain[62] of a direction orientation test for children, I devised one containing 30 different items. Below is a sample sheet of directions for the test, which is given orally. The testees number their answers to correspond to the questions.

ORIENTATION TEST C

Suppose you are in a city where the streets run north and south, and east and west, then

(1) Suppose you are going north, then you turn to your right, what direction are you going now? (5 sec.)
(2) Going north, then L, then R. (6 sec.)
(3) Going west, then R, then R again? (6 sec.)
(4) Going south, then L, then R again? (6 sec.)
(5) Going north, then R, then R again, then L? (7 sec.)
(6) Going south, then L, then R, then L? (7 sec.)
(7) West, then R, then R again, then R again? (7 sec.)
(8) South, then L, then L again, then L again? (7 sec.)
(9) West, then R, then R again, then R again, then L? (9 sec.)
(10) South, then L, then L again, then R, then L? (9 sec.)
(11) East, then R, then L, then R, then L? (9 sec.)
(12) Which way would you have to face so your left hand would be toward the east? (5 sec.)
(13) Which way would you have to face so your left hand would be toward the north-east? (6 sec.)
(14) Which way would you have to face so your right hand would be toward the north-west? (6 sec.)
(15) Which way would you have to face so your left hand would be toward the south-west? (6 sec.)

Suppose you are in a city where the streets run north-east, south-west, and north-west, south-east, then

(16) Suppose you are going north-east, then you turn to your right, what direction are you going now? (5 sec.)
(17) Going south-east, then turn to the left? (5 sec.)
(18) Going north-west, then turn to the right? (5 sec.)
(19) Going south-west, then R, then R again? (6 sec.)
(20) Going north-east, then R, then R again, then L? (7 sec.)
(21) Going south-west, then L, then R, then L? (7 sec.)
(22) Going north-west, then R, then R again, then R again? (7 sec.)
(23) Going south-east, then L, then L again, then L again? (7 sec.)
(24) Going north-west, then R, then R again, then R again, then L? (9 sec.)
(25) Going south-west, then L, then L again, then R, then L? (9 sec.)
(26) Going south-west, then R, then R again, then R again, then R again? (9 sec.)
(27) Which way would you have to face so your left hand would be toward the south-west? (6 sec.)
(28) Which way would you have to face so your right hand would be toward the south-east? (6 sec.)
(29) Which way would you have to face so your left hand would be toward the north-west? (6 sec.)
(30) Which way would you have to face so your right hand would be toward the north-east? (6 sec.)

Write an account of how you carry out the directions.

Are you confused in distinguishing right from left?

Do you have difficulty in making this distinction in such situations as are involved in military drill, waiting on table, driving a car?

[62] L. M. Terman and M. B. Chamberlain, Twenty-three serial tests of intelligence and their intercorrelations, *J. Appl. Psychol.*, 2, 1918, 341-354.

This test as given above was the outcome of considerable preliminary investigation since it was necessary to choose items in such a way as to get a fairly normal distribution of scores. A percentile distribution was made for the scores of 146 unselected women. The median score was 18.8; (Q_3, 24.9; Q_1, 13.16). Seventy-one men gave a much higher median, 24.21.[63]

In order to determine the reliability of the test it was given a second time to 89 women. The coefficient of correlation between the first and second giving of the test is +.78 (p.e. =.044). In order to determine the degree to which success in the orientation test was determined by intelligence the Thorndike scores of 47 women were correlated with the scores on the orientation test. The correlation is low, namely +.124 (p.e. = .096). Differences in general intelligence as measured at present do not greatly influence the scores on the test although no doubt the acquiring of a technique for handling it would serve in a practice series to raise scores for the more intelligent subjects. There are, however, individuals of superior intelligence who are completely baffled by the test and low grade subjects who handle it with perfect ease.

Introspective reports on the way in which the test is manipulated are very interesting and valuable but will not be included in this paper. There are also interesting indications of particular difficulties encountered in certain items of the test, but, again, this material will not be canvassed in this article.

Table XXIII gives a summary of the results for 157 girls so as to show how ease or difficulty in orientation is related to agreement or disagreement between dominant eye and hand. Percentages were calculated for the right-eyed, left-eyed, and either-eyed who are above and below the median and also for those above the 75 percentile rank (Q_3) or below the 25 percentile (Q_1) as determined by the distribution of the scores of the 146 unselected cases mentioned above. Some special effort was made to increase the number of left-eyed individuals in the experimental group. The table confirms the conclusion drawn from the questionary returns received from a totally different group that there is more apt to be confusion between right and left when the dominant hand and eye are not on the same side than when they are.

It is possible that the results might be even more striking if the test was carried out on a younger group since shifts in the originally sighting eye appear to occur frequently in adults on account of visual difficulties. A group of 31 right-handed high school girls (included in the above tabulation) gave the following records in the orientation test.

[63]The test in its present form is not sufficiently difficult for college men. It maybe men have acquired through experience a technique for handling spatial relations that most women lack, or that more men than women are symmetrical in type as, in fact, ny data suggest.

	Right-eyed		Left-eyed	
	No.	%	No.	%
Above Median	18	81.81	3*	33.33
Below Median	4	18.18	6	66.66

*One *S* had defective vision in the right eye.

TABLE XXIII

Orientation Test and Eyedness
157 Girls
Median Score of unselected group = 18.8
$Q_3 = 24.9$; $Q_1 = 13.16$

	Right Eyed		Left Eyed		Either Eyed	
	No.	%	No.	%	No.	%
Above Median	54*	62.79	20	37.74	6	33.33
Below Median	32	37.21	33†	62.26	12	66.67
Total	86	100.00	53	100.00	18	100.00
Above Q_3	34*	61.82	11	33.33	4	40.00
Below Q_1	21	38.18	22	66.67	6	60.00
Total	55	100.00	33	100.00	10	100.00

*One left-eyed, left-handed *S* included.
†One right-eyed, left-handed *S* included.

Mirror-writing and mirror-reading are unquestionably tied up both with the general problem of spatial orientation and with the particular problems that center in types of dextrality. I have at hand some relevant material bearing on these two activities but must reserve it for a later report.[64]

(12) *Temperament and handedness.* As another lead I sought to determine possible relationships between temperament and handedness. In the original questionary on dextrality sent out to psychologists I asked for a self-classification into introvert and extrovert.[65] The concepts are not unambiguous and are besides open to other criticisms. Many respondents refused to classify themselves. Others describe themselves as balanced or mixed in type ('ambiverts'). There were 20 women and 55 men who classified themselves as extroverts, and 18 women and 76 men who classified themselves as introverts When these classifications were studied in the light of the dextrality records, it was found that there was some evidence of a relationship in certain respects.

[64] S. T. Orton in a report on Word blindness in school children, (*Arch. Neur. & Psychiat.*, 14, 1925, 1-35), states that interference with the *visual* associative function results from a unilateral lesion and interprets this to mean that in the case of this function there is no fusion of right and left hemispheral activities. "For some children," he writes, "with the first efforts at learning to read the external visual stimuli irradiate equally into the associative cortices of both hemispheres, and are there recorded in both dextrad and sinistrad orientation." To such a cause he attributes disability in differentiating 'p' from 'q' and 'b' from 'd.' Again, "those children who are neither dominantly right-handed or left-handed, or in whom clear dominance has not been well established before they begin to learn to read, probably have more trouble with reversals of letters (30). Mirror-writing and mirror-reading—to be discussed in a separate report—are tied up with the general probem of spatial orientation and dextrality types.

[65] J. E. Downey, How the psychologist reacts to the distinction 'Extrovert-Introvert,' *J. Abn. Psychol. & Soc. Psychol.*, 20, 1926, 407-415.

The following summary is in point. There were 54 cases in which there was a lack of harmony between preferred hand and eye. Of these 54 cases, 46.3% describe themselves as introverts, 18.5% as extroverts, 20.4% as ambiverts, and 14.8% who refused to classify themselves. The results suggested a connection between eye-hand discrepancy and an introverted make-up. The classification into dextrality types, while it furnished no conclusive evidence, did give some indication that the introvert was more asymmetrical than the extrovert.

In my further study of introversion and extroversion I gave some 100 of the Colgate Mental Hygiene Tests, Personal Inventory C 2,[66] for introversion, to students whose handedness habits I had studied. Laird, who devised this test, furnishes a percentile ranking on the basis of which individual results may be scored. His percentile ranking, however, could not be used with my students since both men and women marked many more introvert characteristics than did his. I have been unable to find the number and the selection of the group on which Laird established his norms. He cites the reliability of the test form as +.85 but does not estimate its validity. My group was composed of students in psychology who had survived the first course in this subject and who were possibly more highly introverted than an unselected group.

Since I was unable to use Laird's norms I ranked my *S*s, men and women separately, in terms of the number of symptoms of introversion checked by each, and I obtained a mean score and the mean variation for contrasting groups. Several of these groups were too small to use, for example, the left-handed and RLL groups, and the men of all formulae. For the women there are strong indications that asymmetry characterizes the introvert. For example, the mean score of 16 RRR women who were also right-eyed was 16.81 (m.v. 3.36) and for 14 RRL women who were left-eyed 21.68 (m.v. 4.44). There were, however, RRL women who were left-eyed who scored low for introversion and a *few* RRR-Right-eyed ones who scored very high. Such *S*s should be carefully studied in order to determine the possibility of acquired eye defects or of a tampering with the native manual habits, but such a study I was unable to carry out. I am tabulating below the formulae of the 11 women who were found by the test to be the most introverted. All of them scored above Laird's ninetieth percentile.

8 RRL_{1-5}–Left-eyed
1 RRL_5–Right-eyed
1 RRR–Left-eyed
1 RRR–Right-eyed

The amount of asymmetry in this group is certainly striking, only 2 are right-eyed, 9 are left-eyed. With 8 of these *S*s I am well acquainted; 7 of them are certainly outstanding introverts, but I should hesitate classifying one (the RRR–Left-eyed *S*) as introverted. I am not acquainted with the one pure dextral *S*.

In contrast to this group are those 11 women with the lowest scores for introversion (below Laird's twenty-second percentile) grouped as follows: 4 RRR–Right-eyed; 1 RRR–Left-eyed; 5 RRL_3–Right-eyed; 1 RRL_3–Left-eyed.

There is less asymmetry in this group than in the former one, only 2 of the women are left-eyed, 9 are right-eyed.

[66] It is interesting to compare these symptoms of introversion with the characteristics cited by the psychologists.

In an earlier section evidence was given that the college women most proficient in the verbal tests were asymmetrical in type, although the proposition must not be reversed. A similar outcome in the test for symptoms for introversion suggests the need of connecting the latter with the former.

In a recent article Bingham has sought to do this.[67] He writes, "it has often proved useful to differentiate people with reference to their relative interest in manipulation, motivating people, and dealing with symbols and ideas." Following Freyd, he finds an excess of introvert personalities among the mechanically inclined and of extroverts among the socially inclined.[68] He also believes that scholars "whose vocation it is to deal primarily with concepts exhibit tendencies toward introversion more frequently than toward extroversion." In discussing the reason for such connection between mechanical or intellectual interest and tendencies toward introversion of personality, Bingham advances the theory "that early introversion of personality leads to the development *through disproportional exercise* of one's native interests in mechanism or ideas, at the expense of interest and proficiency in social contacts."

In the light of the present study I find some difficulty in grouping together as introverts on the same level those interested in dealing with symbols and ideas and those interested in manipulating mechanisms, at least so far as the latter are naturally gifted. I myself should expect those who are mechanically gifted by nature to show a high degree of unilateralization of function in contrast to the asymmetry of the verbally gifted since skill in handling spatial relationships would appear to be a part factor in mechanical achievements. Highly unilateralized individuals are apparently less 'temperamental' than asymmetrical individuals. Quinan found more evidence of asymmetry in musicians than in machinists.[69]

The relationship of introversion to nervous and emotional instability is by no means clear. In a further attempt to clear up my findings I used Woodworth's Personal Data Sheet on one group of 54 women, and Laird's revision of this test, known as the Colgate Mental Hygiene Test, Personal Inventory B 3, with another group of 60 women and studied the outcome in relationship to the handedness formulae. I found, however, very little evidence that asymmetry of function was related to high scores in these tests.

(13) *Handedness and speech defects.* That sinistral tendencies point to the existence of psychopathic trends is as common a belief as that they are indicative of inferior mentality. In fact the two beliefs are different slants on the same problem. Quinan, in particular, has argued that sinistrality is indicative of congenital inferiority of the central nervous system. He uses the word "sinistrality" to designate "partial as opposed to established left-sidedness" and cities as the most significant symptom of this condition "the dominance of the left eye in binocular vision and a limited use of the left hand as the leading hand."[70]

I hoped to be able to make some contribution to this problem in the present study. I can, however, present only a few

[67]W. V. Bingham, Personality and vocation, *Brit. J. Psychol.*, 16, 1926, 354-362.

[68]The personalities of the socially and mechanically inclined, *Psychol. Monog.*, 33, 1924, no. 151.

[69]A study of asymmetry and muscle coördination in musicians, iron-workers, and others, *Arch. Neur. & Psychiat.*, 7, 1922, 360.

[70]See two articles previously cited.

relevant facts and suggestions for further work. Such a study of individuals as is outlined in this paper should be extended to two groups of *Ss*: (1) psychotic and psychopathic *Ss*, and (2) speech cases. There is little material, so far as I am aware, on the dextrality habits of the first group. There is much material as regards the second. But dextrality has been too narrowly conceived in the discussion of speech disturbances and needs to be extended to cover eye habits as well as hand habits.

In the questionary I sent out I sought information concerning speech difficulties and delay in learning to talk. I also obtained the handedness formulae of such speech cases as were available. The margin of error in the questionary returns is undoubtedly very great since obviously certain individuals list as speech disturbances hesitancy and mild emotional blockings which other respondents would overlook. The standard is a fluctuating one. I have, however, included in my tabulation only the serious cases that were reported, including stammering and stuttering, and I have made no use of the reports on delayed speech although this is a fruitful field of work. The reports are tabulated separately for the right and for the left-handed.

Questionary returns from 409 *Ss* were utilized for the right-handed—208 from male psychologists, 93 from delinquent girls whose speech defects were reported by their teachers, 96 from college girls, and 12 from a miscellaneous group of speech cases. Altogether 57 cases of speech disturbances were available for distribution under the handedness formulae of right-handed persons as follows:

TABLE XXIV

Speech Disturbances and Handedness Types

Formula	Right-eyed	Left-eyed	Total	%
RRR	5	10	15	26.32
RRL	16	10	26	45.61
RLL	5	2	7	12.28
R R R L L	4	5	9	15.79
Total	30	27	57	100.00

From the tabulation it is evident that sinistral tendencies, either dominance of the left eye or a total or partial dominance of the left hand in bimanual operations characterizes all but a small percentage of the cases. Since, however, the majority of human beings have sinistral tendencies of one sort or another one cannot conclude that the latter predispose to speech disturbances but only that the pure dextrals are relatively immune. Only 5 RRR–Righted-eyed individuals report speech trouble.

It is interesting to note that 1 of the 5, although RRR in type, claims that he is not strongly right-handed and that 4 of the 5 are left-thumbed.

The percentages for each formula must be reviewed in the light of the percentage of unselected individuals classified under each formula. It appears that the number of speech cases is especially high for the RLL formula and for those individuals

$$\begin{matrix} R & R \\ R & . \\ L & L \end{matrix}$$

of unstabilized manual habits entered as

In determining the percentage of speech disturbances for the right-handed it is necessary to use the unselected group of 390, who gave 45 cases. The calculation runs slightly over 11%.[71]

Let us turn to the speech cases of the left-handed. The questionary returns for them were probably much less accurate than for the right-handed since they were unchecked and received from a miscellaneous group. They err, I believe, in reporting too few cases. The total percentage of speech difficulties was 35 (20.5%) for 171 left-handers distributed as follows:

TABLE XXV

Speech Disturbances and Handedness Types

Formula	Left-eyed	Right-eyed	Total	%
LLL	15	3	18	51.43
LLR	6	5	11	31.43
LRR	1	5	6	17.14
Total	22	13	35	100.00

It appears from the above tabulation that the number of speech cases, 51.43%, contributed to the total by the LLL group, particularly the left-eyed members of this group, is large as compared with the percentage of LLL individuals in the total group of left-handers, about 28%. See Table II. The percentages of cases recorded for LLR and LRR individuals are low.

Such figures need to be confirmed by a more accurate procedure in which speech-diagnoses are made by specialists, and by a more extensive survey. Cases of pathological left-handedness should also be excluded. Taken at their face value they may be compared with the tabulation of the speech difficulties of the right-handed. The striking point of contrast is that while pure dextrals are relatively immune so far as speech disturbances are concerned, pure sinistrals are disproportionally

[71] Smiley Blanton (Speech defects in school children, *Ment. Hygiene*, 5, 1921, 820-827) reports that 18.13% of 1,400 University of Wisconsin Freshmen "were found to be unable to meet the necessities of English speech."

subject to such troubles. Moreover, the RLL individual appears to suffer more frequently from a speech impairment than does the LRR individual.

My own anticipation, confirmed by the tabulation for right-handed but not by that for the left, was that a high degree of unidextrality (pure dextrality and pure sinistrality) would be unfavorable to the development of speech defects, which would appear largely in the mixed types. The outcome is, however, in line with the belief that the greater the tendency to sinistrality the more likely there is to be speech trouble.

It is a common notion with reference to speech and handedness that tampering with the native manual preference induces speech trouble.[72] It is possible that such tampering may have a more disastrous effect on the LLL child than on the LRR or LLR one and that this would explain the high percentage of LLL speech cases in the tabulation just given. Further investigation should emphasize this possibility for it is a matter of considerable importance pedagogically if it should be found that children of the LLR or of the LRR formula may be trained to use the right hand with relative impunity.

I have been impressed in my study of individual cases by the ease with which many LRR individuals shift to the right hand in writing. Recently a teacher of drawing who uses both hands in her work told me that she had shifted from left to right-handed penmanship in the eighth grade without the slightest difficulty. She did this on the suggestion of an older brother who was also left-handed and who had shifted in the same way late in his school career. Her formula was LRR–Right-eyed. The young lady reported that her teacher of psychology informed her that sooner or later she would develop a speech disturbance, a prophecy which was never realized. The young lady reported also that she learns skilled movements with the left hand and then transfers them to the right.

Lack of stabilization of manual function appears to be a bad symptom, but the LRR or RLL individual should not be thought of as unstabilized. The formula for the latter is $\begin{Bmatrix} L\,R \\ R \\ R\,L \end{Bmatrix}$. A common casual factor for both impaired speech and for unstabilized manual habits may be found in instability in the functioning of the nervous system. There is need at present of comprehensive studies of the development of linguistic and manual habits in young children.

A tabulation of the handwriting habits of the majority of my left-handed *Ss* is given in Table XXVI. I wished to determine whether certain left-handed types yield to pressure more readily with regard to writing than others do. Of course the question can be answered only in a very general way since one

[72] L. M. Terman, *Hygiene of the School Child*, 1914, 1-418, chap XIX; also Ballard, *op. cit.*, 299 f. It is interesting to note that investigators of bilingual children report that they have not as unerring a sense of dextrality as the monoglots, which is supposed to be caused by the conflict initiated in learning a second language before they have acquired adequate power of expression in their native language. See D. J. Saer, The effect of bilingualism on intelligence, *Brit. J. Psychol.* 14, 1923, 25-38.

has no measure of the amount of pressure exerted in any particular case. It is, moreover, probable that the pressure upon the younger members of the group was less than that put upon the older members since it is widely believed to-day that it is a mistake to insist that a left-handed child write with the right. One curious difference in the amount of shifting was found in comparison of the superior and inferior groups. Only three of the latter individuals, or 20%, wrote right-handed. But of the superior groups more than 42% wrote with the right hand. Two interpretations of this are possible. Either more effort is made by parents and teachers to change the superior child to

TABLE XXVI
Dextrality Types and Writing Hand

Formula	No.	Writes Right		Writes Left	
		No.	%	No.	%
	Women				
LLL	32	12	37.5	20	62.5
LLR	31	10	32.2	21	67.7
LRR	23	11	47.8	12	51.3
Total	86	33	38.4	53	61.6
	Men				
LLL	31	16	51.6	15	48.3
LLR	29	10	34.4	19	65.5
LRR	64	31	48.4	33	51.6
Total	124	57	45.9	67	54.0
	Combined Men and Women				
LLL	63	28	44.4	35	55.5
LLR	60	20	33.3	40	66.6
LRR	87	42	48.1	45	51.7
Total	210	90	42.8	120	57.1

writing with the right hand or else these individuals discipline themselves. Table XXVI gives the writing habit for 124 men and 86 women and also the total for the two sexes. There is no evidence of a significant difference with reference to the percentage of shift-overs for the different handedness formulae although possibly with larger numbers a significant difference might be found between the LLR and LRR groups since nearly 50% of the latter use the right hand in writing, and only 33.3% of the LLR group. It is possibly significant that the formula LLR gives a low percentage for impaired speech although it is, apparently, the one that shows least frequency in shifts to writing with the right hand.

Summary and Conclusions

The data reported in this paper, gathered from a survey of about 1500 individuals, show impressively the value of considering both unimanual and bimanual performances in the study of dextrality.

Specifically, the three types, RRR, RRL, and RLL, with the corresponding types for the left-handed, LLL, LLR, and LRR, have proved to be most illuminating descriptions of manual habits. The percentage distribution of these types has shown some significant percentage differences existing between superior and inferior men and between men and women.

Contrasting types are characterized by all manner of transitional forms, so that in experimental work a very large number of subjects would be necessary if one were to sift out particular sub-groups for adequate investigation of particular problems. The term 'type' is used as a convenient mode of description. Whether it would be possible to obtain a single curve of distribution running from the pure dextral at one end, through the mixed types, to the pure sinistral at the other, I do not know. Certainly I have no reason to think that the pure sinistral would in number equal the pure dextral. In no investigation should the possibility of sex differences be overlooked, nor the probable existence of pathological as opposed to a natural left-handedness.

The matter of a fixating eye was shown to be important, but it seemed from the present data impossible to believe that as a usual thing the dominant eye determined handedness, or even the reverse. The fact that one thumb rather than the other, is outside when the hands are clasped, with fingers interlocking, seemed to have some significance, since a dominant left thumb appeared to indicate latent sinistrality. Rife's theory that the spade foot is determined by the bimanual dextrality, was confirmed.

Evidence was presented that suggests the possibility of characterizing dextrality groups by structural differences. Differences in bone-measurements and in finger-print patterns for the two hands deserve thorough consideration.

It is also possible that certain motor and psychic differences are bound up with differences in degree of unidextrality.

For *women*, there is some evidence that the following motor and psychic characteristics are correlated with handedness types; the pure dextral appears to be motorly more expert than the mixed types, less subject to speech disturbance and less introverted. The mixed type, and especially the type with asymmetry of eye and hand, give a higher Thorndike score and

a higher score on verbal tests, shows some special disability on the Thurstone and orientation tests, and is often highly introverted.

Comparable records have not been obtained for men, except in the case of the Thorndike scores, where in contrast to the results for women, the unilateralized and ambidextral male appeared to be superior to the mixed type.

Suggested Program for further Investigation

The above conclusions are to be understood as tentative only. The main purpose of the present paper was exploratory, to suggest fields for more intensive experimental work, following the technique herein described. Many problems suggest themselves in a program for work. I am listing a few below, as I hope that the field now opened up may be worked extensively.

(1) The tests described or similar ones should be tried out on a large group of men. It is particularly desirable that the RLL group should be tested. On account of the limited number of women falling in this group, men must be used.

(2) Surveys of handedness types and the amount of asymmetry between eye and hand should be made for large groups of mental defectives and for psychopathic subjects. Dextrality types should also be determined for the congenitally blind.

(3) Studies of different racial groups should be made. I expect to publish a brief report on the handedness of small groups of North American Indians.

(4) Genetic studies are badly needed. Certain problems relative to the relationship existing between hand and eye might be solved by a determination as to which habit, the optical or the manual, is first stabilized. I am giving elsewhere a brief report on observations carried out on 49 preschool children. It is my impression that the manual habits are stabilized earlier than eye habits. The problem of determining the age for stabilization of each may be suggested to Directors of Nursery Schools as a valuable one to work on. The motor and temperamental traits of right-handed but left-eyed children should be determined and also the intelligence of the RLL (or LRR) child. I believe the latter will run high.

(5) Studies in the heredity of handedness need to be reviewed in the light of the handedness types described in this paper.

(6) Vocational applications of the present study remain to be worked out. I have been impressed by the number of skilled typists in my LRR group. I have failed to make records on this point for the RLL individual. I wish to cite in this connection the following recent statement by Book.

"It also is evident from our results that an unusual amount of voluntary motor control in the left hand and arm is needed to attain the highest degree of success in typing. This is indicated by the fact that such superior control in the left hand and arm is present in all individuals that have achieved the greatest success in typing and in learning to typewrite, and by the fact that the mean variation for the left hand movements is less for all these contestants than it is for the right."[73]

A determination of the handedness types of champion typists would be of extreme value in the light of the facts revealed by Book's investigation. A similar study should be made on successful telegraphers. Again, I believe, RLL and LRR men may have an advantage in such work.

The many problems growing out of such a survey as the one reported in the present paper suggest the need of coöperation among investigators.

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[73]W. F. Book, Voluntary motor ability of the world's champion typists, *J. Appl. Psychol.*, 8, 1924, 283-308.

PSYCHOPHYSICAL ANALYSIS

By L. L. THURSTONE, University of Chicago

The purpose of this paper is to present a new point of view in psychophysics and to trace some of its implications. In the determination of a difference limen, the psychophysical judgment, no matter which of the classical methods is followed, is traditionally considered to be a function of *two* factors, namely, (1) the separation or difference between the two physical stimulus magnitudes, and (2) a discriminatory power measured in terms of sense-distances or just noticeable differences. The psychological continuum, no matter what it may be called, is supposedly determined by these just noticeable differences or equal appearing intervals, which are by definition assumed to be equal. The stimulus magnitudes are laid out on this continuum as landmarks, and the psychological separation between them is stated in terms of just noticeable differences or equal appearing intervals.

It will lead to a rather more flexible and illuminating analysis if we start out a little differently. I shall suppose that every psychophysical judgment is mainly conditioned by four factors, namely the two stimulus magnitudes or the separation between them, the dispersion or variability of the process which identifies the standard stimulus, and the dispersion or variability of the process which identifies the variable stimulus. The present analysis will concern these variables and finally the experimental procedures by which they may be isolated.

At the outset it may be well to make clear some things that will *not* be assumed. I shall not assume that the process by which an organism differentiates between two stimuli is either psychic or physiological. I suppose it must really be either, or perhaps both, but it is indifferent for the present argument whether the processes by which we identify or discriminate grays and loudnesses and handwriting specimens are mental or physiological. Hence this analysis has nothing really to do with any psychological system. I shall try not to disturb the main argument with systematic irrelevances or with my personal notions regarding the psychic or physiological nature of the psychophysical judgment.

Further, I shall not assume that sensations, or whatever the identifying and discriminating functions may be called, are magnitudes. It is is not even necessary for the present argument to assume that sensations have intensity. They may be as qualitative as you like, without intensity or magnitude, but I shall assume that sensations differ. In other words, the identifying process for red is assumed to be different from that by which we identify or discriminate blue.

A term is needed for that process by which the organism identifies, distinguishes, discriminates, or reacts to stimuli, a

a term which is innocuous and as non-committal as possible, because we are not now interested in the nature of the process. Sensations, or more generally, subjective conditions would be good terms but physiological states or intraorganic conditions would also be satisfactory. In order to avoid any implications I shall call the psychological values of psychophysics *discriminal processes. The psychophysical problem concerns, then, the association between a stimulus series and the discriminal processes with which the organism differentiates the stimuli.*

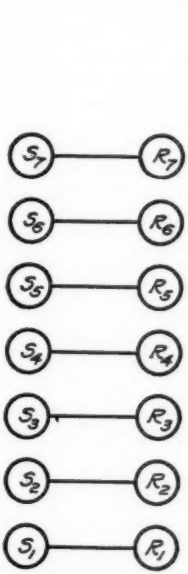

FIG. 1

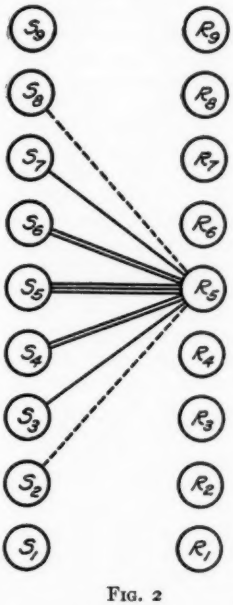

FIG. 2

In Fig. 1 let the circles R_1, R_2, R_3, R_n represent a series of stimuli which constitutes a continuum with regard to any prescribed stimulus attribute. It is not necessary to limit psychophysical analysis to stimuli which have intensity or magnitude as their principal attribute. For example, a series of handwriting specimens may be arranged in a continuum on the basis of general excellence. They would of course arrange themselves in a different continuum if some other attribute were specified such as size of letters, legibility, coarseness of pen, or what not. Similarly a series of spectral colors may be

arranged in a continuum for discrimination of brightness, chroma, saturation, apparent remoteness from red, or what not. Psychologically some of these attributes can be measured, while physically the measurement may even be impossible. We are assuming, then, that a series of stimuli have been arranged in a continuum according to any attribute about which one can say 'more' or 'less' and that psychophysics need not be limited to stimuli which have magnitude or size, such as lifted weights and the brightnesses of grays.

Referring again to Fig. 1, suppose that each stimulus in the series has a discriminal process which is a psychic or physiological function of the organism. Thus the stimulus R_5 has a discriminal process S_5 with which the discrimination of the stimulus takes place. These discriminal processes, whatever be their nature, can be labeled only in terms of their corresponding stimuli so that the discriminal process S_5 is labeled by the stimulus R_5 with which it is associated. In the same manner the other discriminal processes in the series may be labeled by the stimuli which produce them. Naturally the discriminal processes would arrange themselves in a totally different order by changing the attribute of the stimuli by which they are arranged in a continuum. We have then two continua, one for the stimuli and one for the discriminal processes of these stimuli. The stimulus continuum must of course be defined in terms of some definite stimulus attribute. The discriminal continuum is a qualitative one which does not necessarily have either magnitude or intensity.

There is of course no possibility of recording experimentally in any direct way these discriminal processes that correspond to a series of stimuli. It is possible, however, to make some interesting inferences about the psychological continuum indirectly. The stimuli may be used to designate locations in the psychological scale just as though the stimuli, or their names, were used as tags or landmarks in a continuum which has otherwise no identifying marks or mile posts. It is the relative separations between these landmarks on the qualitative psychological continuum which it is the central problem of psychophysics to survey. In the figure there is no attempt to indicate quantitatively the relative separations between the stimuli, or between their psychological correlates. The diagram indicates only that for each of the stimuli in the stimulus continuum one may postulate a discriminal correlate and that these psychological correlates also form a continuum of some kind. Nothing more is known, for the purposes of measurement, about the psychological continuum except that a discrete series of discriminal processes of unknown nature can

be used as landmarks along its course and that these processes or landmarks are experimentally controlable or identifiable only in terms of the physical stimuli that produce them.

So far the argument has proceeded as though there were a fixed one-to-one relation between the stimuli and their respective psychological correlates. It may be assumed that this relation is not so fixed as might be indicated by Fig. 1. It undoubtedly happens that stimulus R_5, for example, does not always produce the same discriminal process S_5. The present method of psychophysical analysis rests on the assumption that constant and repeated stimuli are not always associated with exactly the same discriminal process but that there is some qualitative fluctuation from one occasion to the next in this process for a given stimulus. This raises an interesting possibility. It might happen for example, that stimulus R_5 has ordinarily S_5 as its discriminal process but that sometimes the qualitative fluctuations would spread to S_4 or to S_6. It might even happen, although rather seldom, that the stimulus R_5 would have as its process S_3 or S_7. It should be recalled that each of these processes or qualities is identified by that stimulus which most frequently produces it so that S_4, for example, is habitually associated with R_4 and so on. This is the fundamental idea of the psychophysical analysis of the present paper.

The variability of this connection between the stimulus and its discriminal process works both ways. A given process S_5 would be associated most frequently with R_5 but occasionally also with adjacent and closely similar stimuli in the stimulus continuum such as R_3, R_4, R_6, R_7. Similarly, the stimulus R_5 can be thought of as most frequently associated with the process or quality S_5 but occasionally with the adjacent qualities such as S_3, S_4, S_6, S_7. Since the discrimination between stimuli is made in the processes of the psychological continuum we shall be concerned with the latter of these two regressions, namely the qualitative fluctuations in the discriminal processes that are associated with a constant and repeated stimulus.

The psychophysical relations may be summarized, so far, in the following propositions.

(1) A series of stimuli R_1, R_2, R_3 . . . R_n can be arranged in a continuum, with reference to any prescribed quantitative or qualitative stimulus attribute.

(2) These stimuli are differentiated by processes of the organism of unknown nature and they are designated S_1, S_2, S_3 . . . S_n respectively. Every stimulus R_k is identified by the organism with the process S_k. These processes may

be either psychic or physiological or both. In this discussion they are referred to as the discriminal processes or qualities.

(3) When the discriminal processes $S_1 \ . \ . \ . \ S_n$ are considered in the same serial order as the corresponding stimulus series they constitute what may be called the discriminal continuum or the psychological continuum. This continuum is the correlate of the already postulated stimulus continuum.

(4) It is assumed that the correspondence $R_n — S_n$ is subject to noticeable fluctuation so that R_n does not always produce the exact process S_n but sometimes nearly similar processes S_{n+1} or S_{n-1} and sometimes even S_{n+2} or S_{n-2}. It goes without saying that the numerical subscripts are here used to denote qualitative similarity and that no quantitative attributes are thereby necessarily injected into the discriminal processes. This fluctuation among the discriminal processes for a uniform repeated stimulus will be designated the *discriminal dispersion.*

In Fig. 2 are represented the two continua, one for the stimulus series and one for the corresponding discriminal processes. Let R_5 be one of the stimuli in the stimulus series. It is asssumed that some discriminal process S_5 occurs more frequently with this stimulus than any of the other processes. Hence it is designated the *modal discriminal process* for that stimulus. In this sense S_5 is the modal discriminal process for the stimulus R_5, and so on.

The relative frequencies of the different processes are represented for stimulus R_5 in a rough diagrammatic way. Thus there are three lines connecting R_5 with S_5 to indicate the relation between the stimulus and its modal discriminal process. There are only two lines connecting the adjacent processes with the same stimulus R_5 and this represents the relatively lower frequency of this association. The processes S_3 and S_7 are connected with the same stimulus with only one line to represent relatively infrequent association. Finally the dotted lines represent in the same manner very infrequent association between the processes so marked and the stimulus R_5. The extreme processes without connection with R_5 represent, then, those processes which are so different from the modal process for R_5 that they never occur in association with the given stimulus or that such association would take place only under unusual conditions as affected by practice, fatigue, sensory adaptation, successive or simultaneous contrast, and so on.

The simplest and perhaps the most obvious plan for scaling would be to assign linear values to the discriminal processes, with reference to a given stimulus, inversely proportional to the frequencies with which these processes occur with the given stimulus. With R_5 as the given stimulus in Fig. 2 the reckoning would start with the corresponding modal process S_5 as an origin or datum. For this stimulus the other processes could be assigned distance-values from S_5 inversely proportional to their frequencies of occurrence with the given stim-

ulus. Any plan that might be adopted is subject to experimental test in that the separations between the processes can be scaled with reference to each of the various stimuli. Naturally these scale distances between the processes should remain practically constant, no matter what the stimulus may be, in order to have a valid measuring method. Experimental test shows that the plan just suggested of assigning distance values on the psychological continuum breaks down. It is found that the separations between the processes do not retain stable values when they are determined for different stimuli. Therefore some other plan must be adopted.

The normal probability curve has been so generally abused in psychological and educational measurement that one has reason to be fearful of criticism from the very start in even mentioning it. The only valid justification for bringing in the probability curve in this connection is that its presence can be experimentally tested. The writer has found experimentally that the normal probability curve was not applicable for certain stimuli. In most of the experiments the distributions are reasonably close to normal.

Since the assumption of a normal distribution for the discriminal dispersion can be experimentally verified and limited to those stimulus series where its reality can be tested, it will be reasonable to make this assumption subject to verification in every case. The hypothesis can be stated as follows. *The discriminal dispersion which any given repeated stimulus produces on the psychological continuum is usually normal. The frequencies with which the discriminal processes occur for a given stimulus ordinarily describe a normal distribution when plotted on the psychological continuum as a base.* In experimental practice the procedure is the reverse of this hypothesis because the frequencies are known first experimentally and from these frequencies we *construct* the psychological continuum. The writer has found in several studies that the separation between any pair of processes remains practically constant no matter which of the neighboring stimuli is used as a base for the calculation. Such is not the case, however, when the separation of any pair of processes is assigned values directly or inversely proportional to their frequency of occurrence.

In Fig. 2 where R_5 is chosen as the stimulus we should therefore, according to this hypothesis, assign scale values to the various processes as distances from S_5 as an origin. These distances would be assigned in terms of the standard deviation of the distribution of process-frequencies. There is of course no further unit in terms of which this standard deviation can be expressed. It is itself a unit of measurement because all that we can do with the psychological continuum is to lay off linear separations between the processes proportional to their true value since, so far as we know, there is in the nature of

the case no further absolute unit of measurement for the psychological continuum. But we shall see that it is possible to compare the discriminal dispersions for two stimuli and to determine experimentally the ratio of any two of these dispersions. *Psychological measurement depends, then, on the adoption of one of these dispersions as a base, and the use of its standard deviation as a unit of measurement for the psychological continuum under investigation.*

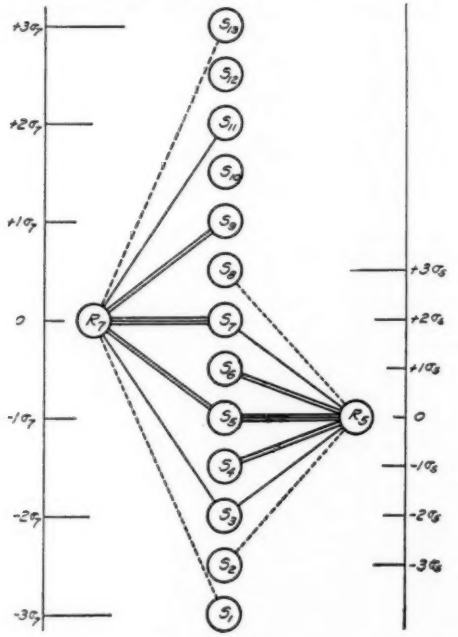

FIG. 3

In Fig. 3 let the column of thirteen circles represent so many discriminal processes, each of them being a modal discriminal process for a stimulus with the same numerical designation. Two of these thirteen stimuli are indicated in the figure, namely R_5 and R_7. Suppose that these stimuli are arranged in a continuum according to any prescribed stimulus attribute and let R_7 be more ambiguous, or less sharply defined, than R_5. An example would be two specimens of handwriting, one of which would be a beautiful but unusual handwriting, or perhaps it might be written in a foreign language,

or it might be in German script which would possibly call forth judgments influenced by prejudice from factors other than those of the handwriting characteristics. If the experiment involves the comparison of loudnesses, a variation of the certainty or ambiguity of judgment for a particular stimulus might be caused by variations in timbre or pitch. Ordinarily psychophysical experiments are so set up as to avoid, as com-

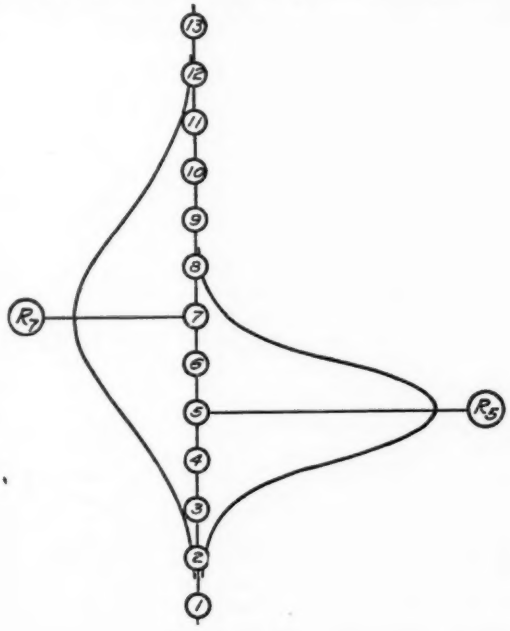

FIG. 4

pletely as possible, the introduction of extraneous factors to influence the ambiguity of judgment and the stimuli are made into as homogeneous a series as may be experimentally possible.

In Fig. 3, the two stimuli are represented as differing in the certainty with which they can be judged as to the prescribed attribute for the stimulus continuum and R_7 is indicated as the more variable or uncertain of the two. The modal discriminal process for R_7 is S_7 as before, and the discriminal processes S_6, S_3, S_1 might be assigned deviation values of 1σ,

2σ, 3σ respectively from S_7 as a datum. These deviation values would be assigned on the basis of the frequency with which each of these processes occur with R_7 as a stimulus. With the same diagrammatic representation let the other processes be assigned their deviation values from S_7 as a base and let the same processes be assigned frequency-deviation values from S_5 as a base for stimulus R_5. In Fig. 3 these hypothetical deviations are given numerical values. Note from the figure that the discriminal process S_5 which is modal for stimulus R_5 has a deviation value of $-1\sigma_7$ for stimulus R_7. Similarly the discriminal process S_4 has a deviation value of $-1.5\sigma_7$ for stimulus R_7 while it has a deviation value of $-1\sigma_5$ for stimulus R_5. If this analysis is correct it should happen not infrequently that the stimuli which constitute a continuum according to any prescribed stimulus attribute are subject to varying degrees of dispersion when they are perceived or judged. Some stimuli are probably placed with reference to the prescribed attribute more accurately and consequently with a smaller discriminal or subjective dispersion than other stimuli. It is probably true that this variability of the discriminal dispersion on the psychological continuum is of relatively less serious importance in dealing with strictly homogeneous stimulus series but it becomes a serious factor in dealing with less conspicuous attributes or with less homogeneous stimulus series such as handwriting specimens, English compositions, sewing samples, Oriental rugs. In measurements of the type known as judgment scales the discriminal dispersion on the psychological continuum becomes one of the unknowns to be determined as well as the scale value of the specimen. Every specimen in such a series presents two unknown values to be determined: namely, the scale value of its modal discriminal process on the psychological continuum, and its discriminal dispersion.

Instead of the diagrammatic representation of Fig. 3 two normal probability curves may be substituted, subject of course to subsequent experimental verification. This has been done in Fig. 4. Here the psychological continuum has been constructed on the hypothesis that the discriminal processes describe a normal distribution when plotted on that continuum. When R_7 and R_5 are presented for a comparative judgment, each of the stimuli produces a discriminal process of some kind and the certainty of the discrimination may be assumed to be mainly a function of the difference between these two processes. If R_7 happens to be associated with one of the processes at the upper range of its discriminal dispersion and if R_5 happens to be associated with one of the processes at

the lower end of its discriminal dispersion, then the discrimination is made with ease and the judgment is correct. If these conditions are reversed so that R_7 has a process slightly below its modal process while R_5 happens to have a process slightly above its modal process, then the two stimuli may even have the same discriminal process and there would be no possibility of a confident discrimination. Finally, if on some occasion R_7

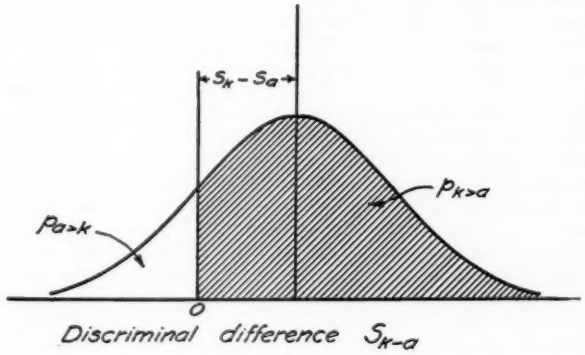

FIG. 5

happens to have a process unusually low in its scale, while R_5 has a process higher in the psychological scale, then the judgment would be made, perhaps even with confidence, that R_5 is greater than R_7 and the judgment would be recorded as incorrect. The discrimination is considered, then, as a function of the *discriminal difference* between the two processes that happen to be associated in the same judgment. By the discriminal difference is meant the linear separation on the psychological continuum between the two processes involved in any particular judgment. It may be designated $S_{7.5}$ or more generally S_{ka}. The discriminal difference is the same as the *sense distance* if we allow that the sense distance for two stimuli fluctuates from one occasion to the next.

If in a long series of experimental judgments it were possible to isolate the two discriminal processes for every judgment and if the separation between these two processes for every judgment were recorded, one could tabulate them in the form of a frequency table of discriminal differences. These differences would of course be expressed in terms of some unit of measurement on the psychological continuum. Let the standard stimulus be A and the variable stimulus K. The mean

of the distribution of discriminal differences would be the mean or true difference $(S_k - S_a)$ and its standard deviation would be

$$\sigma_{ka} = \sqrt{\sigma^2_k + \sigma^2_a}$$

on the assumption that deviations from the modal processes for the two stimuli are not correlated. This distribution is represented in Fig. 5. The base line of this distribution represents discriminal differences in terms of any desired unit of measurement on the psychological continuum. The mean is $(S_k - S_a)$ because that is the difference between the two modal processes. The origin represents a difference of zero. This would occur when the two stimuli happen to be associated with the same discriminal process in which case there is no discrimination possible. The points to the right of the origin on the base line represent positive values for the differences S_{k-a} in which R_k has a process higher in the scale than R_a. Similarly the points to the left of the origin represent negative values for the discriminal difference S_{k-a} in which R_k happens to have a process lower in the psychological scale than R_a. It should be recalled that S_{k-a} or S_{ka} represents the sense distance between two stimuli on any particular occasion whereas $S_k - S_a$ represents the mean sense distance for several hundred judgments and it is in scale construction called the 'true' sense distance or scale distance between the two stimuli.

For the present it will simplify analysis to assume that any discriminal difference, no matter how small, is directly reflected in the judgment. A correction may be inserted for this approximation by which a *discriminal difference limen* can be calculated but this correction will not seriously alter the results. It may be assumed for the present that all positive discriminal differences, S_{k-a}, result in the judgment "R_k greater than R_a" and that all negative discriminal differences result in the judgment "R_a greater than R_k." If the two paired stimuli are presented N times there will of course be observed N discriminal differences and their expected distribution is represented in Fig. 5. The shaded portion of that figure represents the expected proportion of judgments "R_k greater than R_a" and these judgments would be correct if K is higher than A. The unshaded portion of the surface represents the expected proportion of judgments "R_a greater than R_k." The proportion of correct judgments will of course increase if the two stimuli are chosen farther apart. Also, the proportion of correct judgments will increase if stimuli are chosen with smaller discriminal dispersions. If the shaded

area is greater than $N/2$ it represents correct judgments. If it is less than $N/2$ it represents the proportion of incorrect judgments.

A discriminal difference, S_{k-a}, is not necessarily a magnitude. It is a pair of processes, a pair of qualities. The only way in which numerical value is assigned to it is by placing each of these processes on a measured continuum by means of the frequency with which each of them is associated with the same stimulus. The difference between these two assigned linear values is the discriminal difference, S_{k-a}. The scale distance, $S_k - S_a$, can be defined as the most common discriminal difference, S_{k-a}.

At this point we have arrived at a measure which can be experimentally verified. By the method of constant stimuli it is readily possible to ascertain the actual proportion of judgments "R_7 greater than R_5" for the two stimuli. This proportion is a function of four variables namely S_7, S_5, σ_7, σ_5. If there are n stimuli in the stimulus series there will be $2n$ unknowns to be evaluated, namely n scale values for the modal discriminal processes, and n scale values for the discriminal dispersions. If every stimulus is used in turn as a standard the number of possible pairs of stimuli will be

$$T = \frac{n(n-1)}{2}. \qquad [1]$$

Since there is an experimental proportion 'a greater than b' for every possible pair of stimuli, it follows that there will be $n(n-1)/2$ observation equations and $2n$ unknowns. One of the modal discriminal processes can be chosen as a datum or origin for the psychological scale, and one of the discriminal dispersions can be chosen as a unit of measurement for the construction of a psychological scale. This reduces the number of unknowns to $(2n-2)$ or $2(n-1)$.

TABLE I

Number of stimuli in the series, n	Total number of unknowns, $2(n-1)$	Number of observation equations, $T = n(n-1)/2$
1	0	0
2	2	1
3	4	3
4	6	6
5	8	10
6	10	15
7	12	21
8	14	28
9	16	36
10	18	45

Table I shows for stimulus series of varying length from 1 to 10, the number of available observation equations and the total number of unknowns. When the stimulus series has less than four stimuli, the number of unknowns is greater than the number of observations equations and the problem therefore cannot be solved. When there are four stimuli in the series the number of observation equations exactly equals the total number of unknowns and the problem can then be solved by simultaneous equations. When the stimulus series has more than four stimuli there are more observation equations than there are unknowns and the problem must then be solved by the method of least squares or by some other method of balancing errors of observation.

The fundamental psychophysical equation can then be stated in the following form.

$$S_k - S_a = X_{ka}\sqrt{\sigma_k^2 + \sigma_a^2} \qquad [2]$$

in which S_k and S_a are the two modal scale values on the psychological continuum for the two stimuli R_k and R_a.

X_{ka} is the sigma value for the experimentally observed proportion of judgments "R_k greater than R_a." When these proportions are greater than .50 the stimulus R_k is higher in the psychological continuum than R_a.

σ_k = the discriminal dispersion of R_k on the psychological continuum.

σ_a = the discriminal dispersion of R_a on the psychological continuum.

The assumptions underlying this psychological equation are as follows:

(1) That every stimulus in the stimulus-series is associated with a modal discriminal process with which the organism identifies the stimulus for a prescribed attribute.

(2) That the modal discriminal process for any given stimulus retains at least some of its identity even when the stimulus is combined with other stimuli into a single perceptual judgment.

(3) That the modal processes may be arranged in a linear psychological continuum in the same serial or rank order as the corresponding stimulus series.

(4) In addition to arranging the discriminal processes in rank or serial order, *linear separations* between them are assigned on the assumption that the discriminal dispersion for any stimulus is normal on the psychological continuum. This assumption is subject to experimental verification.

(5) That the discriminal deviations for the different stimuli are uncorrelated. This a fairly safe assumption but if they are correlated, the psychophysical equation [2] becomes

$$S_k - S_a = X_{ka}\sqrt{\sigma^2_k + \sigma^2_a - 2 \cdot r_{ka} \cdot \sigma_k \cdot \sigma_a}$$

in which case the numerical solution becomes unwieldy.

(6) That all positive discriminal differences S_{k-a} give the judgment '$k > a$,' that all negative discriminal differences S_{k-a} give the judgment '$k < a$,' and that discriminal differences of zero, $S_{k-a} = O$, are equally distributed between 'higher' and 'lower' if only two judgments are allowed. This is a close approximation to truth but a correction can be introduced in terms of a discriminal difference limen for judgments 'equal' and 'doubtful.' This correction is left for a separate paper.

Experimental Procedure for Verifying Assumed Normality of Discriminal Dispersion

Assumption (4), that the discriminal dispersion for any stimulus is normal on the psychological continuum, may be experimentally tested by ascertaining whether the separation between any two modal processes (sense distance) remains constant no matter which of the stimuli is used as a base. Consider R_k as the base or standard for equation [2]. Then the proportion of judgments $k > a$ will be controlled by the relation

$$S_k - S_a = X_{ka}\sqrt{\sigma^2_k + \sigma^2_a} \qquad [2]$$

Similarly for the proportion of judgments $k > b$,

$$S_k - S_b = X_{kb}\sqrt{\sigma^2_k + \sigma^2_b}$$

Subtracting,

$$S_b - S_a = X_{ka}\sqrt{\sigma^2_k + \sigma^2_a} - X_{kb}\sqrt{\sigma^2_k + \sigma^2_b} \qquad [3]$$

If the same equation is written with R_l, R_m, R_n, as standards, we have

$$\begin{aligned} S_b - S_a &= X_{la}\sqrt{\sigma_l^2 + \sigma_a^2} - X_{lb}\sqrt{\sigma_l^2 + \sigma_b^2} \\ &= X_{ma}\sqrt{\sigma_m^2 + \sigma_a^2} - X_{mb}\sqrt{\sigma_m^2 + \sigma_b^2} \\ &= X_{na}\sqrt{\sigma_n^2 + \sigma_a^2} - X_{nb}\sqrt{\sigma_n^2 + \sigma_b^2} \end{aligned}$$

If every separation such as $S_b - S_a$ remains constant when determined by different stimuli such as R_k, R_l, R_m, R_n, as standards, then internal consistency for the measurements has been demonstrated and the validity of assumption (4) is thereby established. Such internal consistency depends on the nature of the assumed distribution of discriminal processes by which the psychological continuum is constructed.

The point of view that I am describing has many implications bearing on well known psychophysical principles. One of the conclusions to which the present analysis leads is that Fechner's law and Weber's law are really

independent and that it is consequently incorrect to speak of these two laws jointly as 'the Weber-Fechner law.' Another important conclusion relates to the well known hypothesis that equally often noticed differences are equal. The present analysis shows that hypothesis is incorrect because it is possible for two differences to *seem unequal* on the average and yet be equally often discriminated. Other implications concern the limitations of the phi-gamma hypothesis in psychophysical experimentation and the distribution of judgments of equality. A few applications of the concept of discriminal dispersion are described below.

Fechner's Law

Fechner's law is usually phrased as follows:

$$S = K \log R$$

in which S represents sensation intensity which we have here called scale value. The notation R refers to stimulus intensity or magnitude. It will be noticed that in writing our psychophysical equation nothing has been said about stimulus magnitudes or intensities because of the fact that many stimulus series that are subjected to psychological measurement are not capable of quantitative measurement on their objective side. For example, the relative excellence of a series of handwriting specimens may be measured on a psychological continuum but the corresponding physical 'magnitudes' probably do not exist as a single variable. The physical handwriting specimens cannot be readily measured as to the stimulus variable 'excellence.'

Fechner's law can be applicable only to those stimulus series in which the attribute which is being judged can also be physically isolated. Then, if the discriminal separations of the psychological continuum are plotted against the physical stimulus attribute and if this plot is logarithmic, Fechner's law is verified.

In many cases there is no possibiltiy of making sure that the physical variable really corresponds to the psychological one. For example, a series of circles can be arranged in a stimulus series in accordance with their diameters. The discriminal experiments may then be carried out with instructions to indicate which of two exposed circles is the larger without specifying further what is meant by larger. The circles would no doubt arrange themselves in the same serial order in the psychological continuum as in the stimulus continuum so that the two series would have exactly the same rank orders. Now, if we want to verify Fechner's law, we should plot the separations between the modal processes for the circles along the psychological continuum against the corresponding physical stimulus variable. Shall we plot diameters on the base line or shall we plot areas? These two plans would arrange the stimulus series in the same rank order, but the re-

lation between diameter and area is not linear. Both diameter and area would be physical variables covariant with the apparent bigness of the circles. Now, if Fechner's law is verified for one of these physical variables, it could not possibly be verified for the other because of the non-linear relation of diameter and area. If we should find experimentally that Fechner's law is satisfied by plotting the psychological continuum against the diameters, for example, that would not justify the conclusion that Fechner's law applies. We could artificially force Fechner's law everywhere by merely selecting that particular stimulus variable which does give a logarithmic relation with the psychological continuum. Fortunately the law has been shown to hold true for many stimulus series in which there is hardly any possibility of an ambiguous stimulus variable and its universality therefore commands our respect.

Weber's Law as Independent of Fechner's Law

In the present discussion Weber's law is interpreted broadly for the frequently observed relation between the stimulus magnitudes and the scale distances on the psychological continuum. I am not here limiting myself to those particular applications of the law by which it is restricted to sensory intensities. The law is not always verified for sensation intensities, but, on the other hand, I have found it applicable to some other stimulus series that are not sensory intensity magnitudes. The present discussion of Weber's law concerns the functional relation between stimulus magnitudes and psychological scale distances without implying that the law is limited to sensory stimulus intensities.

Weber's law and Fechner's law are often described together and they are frequently called jointly 'the Weber-Fechner law.' The two laws are independent so that either one of them may be applicable without the other being verified for a particular set of data. The two laws must be separately verified for any given set of data.

Weber's law is usually stated as follows: The just noticeable increase of a stimulus is a constant fraction of the stimulus. The term 'just noticeable' is ambiguous so that it is necessary to specify how often a stimulus increase must be correctly noted in order for the stimulus increase to be called 'noticeable.' This frequency is often placed arbitrarily at 75% of the judgments when two judgments are allowed. Restating Weber's law with this provision so as to remove the ambiguity of the term 'just noticeable' we have the following statement of the law: The stimulus increase which is correctly discriminated in 75% of the attempts, when only two judgments 'higher' and 'lower,' or their equivalents, are allowed, is a

constant fraction of the stimulus magnitude. With reference to Fechner's law there are two cases under which Weber's law may be verified. In case 1 Fechner's law is postulated, and in case 2 it is not postulated.

Case 1. Let the stimulus magnitude be designated R_a and let it be increased to the magnitude R_b at which separation the two stimuli are correctly discriminated in 75% of the attempts by the constant method and with two judgments allowed. At this separation between the two stimuli our psychophysical equation [2] takes the following form:

$$S_b - S_a = X_{ab}\sqrt{\sigma_a^2 + \sigma_b^2}$$

which, when stated explicitly for the required proportion of 75% judgments "b greater than a," becomes

$$\frac{S_b - S_a}{\sqrt{\sigma_b^2 + \sigma_a^2}} = X_{ab} = 0.674. \qquad [4]$$

Weber's law states that any pair of stimuli, R_a and its increased magnitude R_b, corresponding to the two modal processes S_a and S_b in the above equation, are such that the fraction R_b/R_a remains a constant no matter what the absolute magnitudes of the stimuli may be. It is clear from the above equation that the separation between the two stimuli which gives a result of 75% correct judgments is a function not only of the two stimulus magnitudes and their corresponding modal processes but also of the discriminal dispersions for the two stimuli. Weber's law may be verified under *Case 1* if an additional condition is satisfied, namely, that the discriminal dispersions are the same for all the stimuli. If the discriminal dispersions are not constant, then it is possible for Fechner's law to be applicable when Weber's is not. If the discriminal dispersions are equal for all the stimuli, then equation [4] may be written as follows:

$$\frac{S_b - S_a}{\sigma\sqrt{2}} = X_{ab} = 0.674 \qquad [5]$$

and since the discriminal dispersion which is here assumed to be constant may be taken as a unit of measurement on the psychological continuum, we have

$$\frac{S_b - S_a}{\sqrt{2}} = 0.674$$

$$\text{or } S_b - S_a = \sqrt{2}\cdot 0.674. \qquad [6]$$

This relation is obtained by condition (2) above. But Weber's law states a constant relation in terms of the stimuli. This transformatinon can be made by Fechner's law as follows:

$$S_a = K \log R_a$$
$$S_b = K \log R_b$$
$$S_b - S_a = K [\log R_b - \log R_a]$$
$$S_b - S_a = K \log \frac{R_b}{R_a} \qquad [7]$$

From equations [6] and [7] we have

$$S_b - S_a = \sqrt{2} \cdot 0.674 = \mathrm{K} \log \frac{R_b}{R_a}$$

or simply

$$\log \frac{R_b}{R_a} = \text{constant}$$

and hence

$$\frac{R_b}{R_a} = \text{constant}$$

thus verifying Weber's law. But in order to verify Weber's law under *Case 1* it was necessary to make two assumptions, namely that Fechner's law applies and also that the discriminal dispersions are constant. If stimuli were used of varying degrees of homogeneity or ambiguity the discriminal dispersions would not be constant and it would then be possible to discover that Fechner's law is applicable when Weber's law is not.

Case 2. It is possible for Weber's law to be applicable when Fechner's law is not verified and when the discriminal dispersions are not all equal. This is best illustrated by a short list of stimuli with hypothetical discriminal dispersions. For the purpose of this illustration we can assume any relation between S and R except the logarithmic relation of Fechner's law. Let us tabulate some paired values for S and R such that $S = R^2$. This is clearly, then, a case in which Fechner's law does not apply. In Table II the first column identifies the six stimuli in the hypothetical series. Column R designates the stimulus magnitudes. Column S shows the scale values of the cor-

TABLE II

Stim. Series	R	$S = R^2$	σ	$P_{(R+1)>R}$
1	10.00	100.	20.00	.75
2	11.00	121.	23.94	.75
3	12.10	146.	29.16	.75
4	13.31	177.	35.40	.75
5	14.64	215.	43.40	.75
6	16.11	259.	51.90	.75

responding modal processes (sensation intensities). Column σ shows a hypothetical series of discriminal dispersions. By means of the fundamental psychophysical equation [2] it can then be shown that the stimuli 1 and 2 are correctly discriminated in 75% of the judgments, that stimuli 2 and 3 are correctly discriminated in 75% of the judgments, and so on. Since the ratio of each stimulus magnitude to the next lower stimulus magnitude is always 1.10 in this table and since these successive pairs of stimuli are correctly differentiated in 75% of the observations, we conclude that Weber's law has been verified by these hypothetical data. The only new factor that we have introduced is the plausible assumption that the discriminal dispersion may not be constant throughout the whole stimulus range. With an assumed variation in the discriminal dispersion we find that it is logically possible to have a set of data in which Weber's law is verified but in which Fechner's law is not verified. All that is necessary for the discriminal dispersion to vary from one stimulus to another is that the stimuli be unequal in the ambiguity or difficulty with which they are judged and this surely must happen much more often than we suspect when the stimuli consist in such qualitative values as handwriting specimens or specimens of English composition. It is quite probable that the variation in discriminal dispersion is rather slight and perhaps negligible when the stimulus series is rather homogeneous. A good example of a homogeneous stimulus series is a set of cylinders for the lifted weight experiment in which size, color, texture, shape, and even temperature are ruled out of the experiment by keeping them constant. In such experiments it is probable that the discriminal dispersion stays constant.

Finally, if the discriminal dispersions can be assumed to be equal throughout the whole stimulus range, then Fechner's law and Weber's law become identical. The frequent association of these two laws as though they were always identical depends on the constancy of the discriminal dispersion. It may be expected in psychophysical experiments with stimuli that are not experimentally kept constant in all but one stimulus variable, that one or two stimuli in the series are more difficult to judge than the rest. In such a case these one or two stimuli will have larger discriminal dispersions than the other stimuli and the consistency of the psychological continuum is thereby disturbed if these variations are not accounted for in the derivation of the scale values.

Equally Often Noticed Differences are not Necessarily Equal

It is usually assumed that equally often noticed differences are equal on the psychological continuum. They are rarely assumed to be equal on the stimulus continuum. It is however incorrect to assume that pairs of stimuli are equally distant on the psychological scale even though all the pairs are equally often discriminated. It is not even correct to say that stimulus differences *seem* equal, or that they are subjectively equal, just because the differences are equally often noticed. Two pairs of stimuli may be equally often discriminated while one of the separations may on the average actually *seem* greater than the other.

Referring again to the psychophysical equation [2] the psychological or *apparent* separation between two stimuli R_a and R_b is expressed by the difference $(S_b - S_a)$, measured on the psychological scale which is a scale of appearances or impressions. The frequency with which the two stimuli can be discriminated is, however, a function of their respective discriminal dispersions as well as their modal discriminal processes. The separation between the modal processes can also be called the mean sense distance. Here again, if we can assume that the discriminal dispersions are constant, then it is correct to say that equally often noticed differences are psychologically equal but that assumption should be tested before constructing a psychological continuun or scale by means of this assumption.

A Possible Effect of Practice

It is probable that practice has the effect of reducing the the discriminal dispersions and that this may account for the shifts in the proportions of correct judgments in psychophysical experiments. If two stimuli are presented to an unpracticed subject for whom these stimuli have relatively large discriminal dispersions, the denominator of equation [4] will be relatively large while the numerator remains constant. Graphically the situation can be represented in Fig. 5 by increasing the standard deviation of that probability curve while the separation $(S_b - S_a)$ remains constant. This produces a low proportion of correct judgments. With practice, the subject reduces the discriminal dispersions and this might be represented in Fig. 5 by reducing the standard deviation of that curve while the separation between the two modal processes remains constant. The effect is to increase the proportion of correct judgments. Naturally, stable results for the construction of a psychological scale depend on reaching such a practice level that the discriminal dispersions will remain prac-

tically constant throughout the experiments. The interpretation of the psychophysical equation in connection with the effect of practice would be that two lights, for example, seem just about as bright to the practiced laboratory subject as to an unpracticed subject. Practice in psychophysical experimentation does not make one of the lights seem brighter or the other one weaker. The two lights retain their same general level of brightness except for sensory adaptation and contrast which are momentary effects. But there is a practice effect in the capacity to discriminate between the two lights. This is determined by the discriminal dispersion or subjective observational error. Here again, equally often noticed differences are not necessarily equal subjectively or psychologically.

Experimental Test

The simplest experimental procedure for verifying the assumption that the discriminal dispersions are constant for any particular stimulus series is probably to arrange a table showing the proportion of judgments, $P_{a>b}$, for all the possible pairs of stimuli. If there are N stimuli, such a table will contain $N(N-1)$ entries if identical stimuli are not experimentally compared. From such a table the stimuli can readily be arranged in rank order. From the table of proportions of judgments, a corresponding table of sigma values can be prepared. One can then plot a graph for X_{ka} against X_{kb} in which a and b are standards. If the discriminal dispersions are equal through the stimulus series, the graph should give a linear plot with a slope of unity. This may be demonstrated as follows:

If in the psychophysical equation

$$S_k - S_a = X_{ka}\sqrt{\sigma_k^2 + \sigma_a^2} \qquad [2]$$

we assume that the discriminal dispersions are equal, the equation becomes

$$\begin{aligned} S_k - S_a &= X_{ka}\sqrt{2\sigma^2} \\ &= X_{ka}\cdot\sigma\cdot\sqrt{2} \end{aligned} \qquad [5]$$

and if we use the discriminal dispersion as a unit of measurement on the psychological scale, we have

$$S_k - S_a = X_{ka}\cdot\sqrt{2} \qquad [6]$$

By symmetry it follows that

$$S_k - S_b = X_{kb}\cdot\sqrt{2} \qquad [7]$$

Subtracting and transposing,

$$X_{ka} = X_{kb} + \frac{S_b - S_a}{\sqrt{2}} \qquad [8]$$

This equation is in linear form and if X_{ka} is plotted against X_{kb} we should have a linear plot. The slope should be unity and

$$Y\text{-intercept} = \frac{S_b - S_a}{\sqrt{2}} \qquad [9]$$

If the plot is linear, it proves that the assumed normal distribution of discriminal processes is correct. If the slope is unity, it proves that the discriminal dispersions are equal. It is left for a separate paper to apply this method to educational judgment scale data.

LINEAR INTERPOLATION *VS.* THE CONSTANT PROCESS

By SIDNEY M. NEWHALL, Yale University

Much has been done to simplify the application of the method of constant stimuli. Computation has been facilitated by the publication of Urban's Tables[1] and later of various checking tables,[2] by detailed accounts of procedure,[3] and by the suggestion of various minor aids.[4] Given the values of the stimuli and the observed frequencies, a threshold can be computed in about 30 minutes provided the first figures check and the computor is familiar with the task.

On the experimental side, certain technical variations in instruction, recording and apparatus have resulted in decreasing the time necessary for obtaining the subject's judgments. Furthermore, it has been shown for lifted weights at least, that the number of the comparison stimuli can be reduced from 7 to 5 without substantially modifying the constants of the psychometric functions and without necessitating a greater number of series.[5]

The requisite number of series is itself a problem and depends on a number of factors such as the training of the subject, his attitude and sensitivity, and the nature of the stimuli. There is experimental evidence that the value of the measure of precision and the approximation of observed frequencies to the phi-gamma hypothesis increase with practice, especially if the subject at the start is untrained in laboratory observation.[6] It is possible then that 200 series, say, may be necessary in one situation, while as few as 20 may be sufficient in another, though it is quite impossible at present to make accurate predictions in the matter. It has been pointed out, too, that the certainty required is dependent upon the nature of the problem.[7] Were a highly 'significant' difference between two limens required, for example, it might be necessary to employ a relatively large number of series to obtain the requisite reliability. The same is true for the commoner problem of securing the usual reliability for a small difference.

Another approach to the problem of simplifying the process of obtaining an accurate limen is to compare different psychophysical methods and results secured by using them. Certain comparisons have been made

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[1] F. M. Urban, *Arch. f. d. ges. Psych.*, 24, 1912, 240 f.

[2] G. J. Rich, this JOURNAL, 29, 1918, 120 f.
L. B. Hoisington, this JOURNAL, 33, 1922, 244-246.

[3] E. B. Titchener, *Quantitative Manual.*
E. G. Boring, this JOURNAL, 28, 1917, 280-293.
W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, 1921.

[4] For instance, suggestions concerning the use of calculating machines, the probable loci of errors in computation, and forms to guide the calculation. The writer has used a mimeographed form in computing a large number of limens. This was suggested by Boring, *op. cit.*, 284, and differs from that in that columns 9 to 15 are replaced by a single column headed 'Rich's Check' which simplifies the checking of the Urban's Tables entries. Furthermore, the formulae for L and h are clearly indicated in the form, minimizing errors in substituting the numerical values.

[5] S. W. Fernberger, this JOURNAL, 25, 1914, 121-130; and also *Psychol. Rev.*, 21, 1914, 335-355.

[6] L. B. Hoisington, this JOURNAL, 28, 1917, 588-596.
Brown and Thomson, *op. cit.*, 1921, 90 ff.
S. W. Fernberger, this JOURNAL, 34, 1923, 498-500.
E. Culler, *J. Exp. Psychol.*, 9, 1926, 169-190.

[7] E. G. Boring, this JOURNAL, 27, 1916, 315-319.
S. W. Fernberger, *Psychol. Bull.*, 14, 1917, 110.

though not always with respect for this problem. Urban, for instance, found the phi-gamma hypothesis to be more satisfactory than the arc-tan.[8] Thomson and Fernberger each found it possible to obtain comparable results by the methods of limits and of constant stimuli.[9] Irrespective of theoretical difficulties, other methods have been presented which employ a simple average value based upon all the data.[10] Various methods of interpolation of the limen have been suggested, sometimes as complete methods and sometimes as accessory to more elaborate methods.[11]

Problem and Method

Simple linear interpolation, as far as the writer is aware, has never been extensively compared in its application with theoretically more precise methods of computation. It has the obvious advantage of a negligible amount of calculation, and it is clear that if it were found to give results as precise as those of the constant process its use would be widely indicated; or if the results were only approximately as accurate the requirements of many problems would be met. Preliminary study of a limited amount of data led the writer to believe the problem worthy of more extended investigation. The problem of present report is, then, to estimate the adequacy of several simple interpolation methods by trying them out on actual data and comparing results with those of the constant process.[12]

As Urban has recently pointed out, there are abundant lifted weight data in the literature which might well be utilized for problems of this character. The writer selected, from among the data of three lifted weight investigations, 100 psychometric functions believing them to constitute a fairly representative sample.[13] The data are of 15 *S*s who varied in respect of age, sex, practice, attitude or instruction, length of session,

[8] F. M. Urban, *Psychol. Rev.*, 17, 1910, 257-9 with reference there given.

[9] Brown and Thomson, *op. cit.*, 1921, Ch. 2.
G. H. Thomson, *Brit. J. Psychol.*, 5, 1912-13, 226 ff.
S. W. Fernberger, *Psychol. Monog.*, 14, 1912-13, No. 4.

[10] G. E. Müller, *Die Gesichtspunkte und Tatsachen der psychophysischen Methodik*, 1904.
C. Spearman, *Brit. J. Psychol.*, 2, 1906-8, 227-242; but cf. Brown and Thomson, *op. cit.*, 61 ff.
R. Pauli, *Psychologisches Praktikum*, 1923, 34-37.

[11] Brown and Thomson, *op. cit.*, 60 f.
E. B. Titchener, *Student's Manual—Quantitative*, 95 f.
A. Lehmann, *Arch. f. d. ges. Psychol.*, 8, 1905-6, 484 ff.
F. M. Urban, *Psychol. Rev.*, 17, 233 ff.

[12] I am greatly indebted to Professor Boring for a number of suggestions concerning both the investigation and the manuscript.

[13] F. M. Urban, Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, *Arch. f. d. ges. Psychol.*, 15, 1909, 268-271. Presented here are 102 sets of p-values, each set based on 50 judgments with each of seven comparison stimuli. Eliminating the extreme upper and lower p-values, I combined intermediate pairs, presumably obtained under comparable conditions, and so obtained sets based on 100 judgments at each of five stimulus values. Thirty-five such sets were selected for the present study, *i.e.* the first thirty-five in Table I.

S. W. Fernberger, On the elimination of the two extreme intensities of the comparison stimuli in the method of constant stimuli, *Psychol. Rev.*, 21, 1914, 342, 351. Presented here are 80 sets of p-values, each set based on 100 judgments with each comparison stimulus. Thirty-seven sets were selected from the "reduced series observed," *i.e.* series in which but five stimulus values were used experimentally.

E. S. Rudisill, Constancy of attitude in weight perception, this Journal, 36, 1925, 576, Tables 1-4. Presented here are 144 sets of p-values each set based on 50 judgments with each of seven comparison stimuli. I proceeded in the same manner as with the Urban data to obtain sets based on 100 judgments at each of five stimulus values. Twenty-eight such sets were selected for use in the present investigation.

and other possibly less important matters. In regard to constants, it can be said that each set of frequencies resulted from 100 series using five comparison stimuli, 88, 92, 96, 100, 104 grm., and a standard of 100 grm. The observed frequencies, forming the basis for all our computations, are given in Table I, to which reference will again be made.

TABLE I
OBSERVED FREQUENCIES

The capital letters signify experimenters and the numbers following subjects, *e.g.* U3 = Urban's *S* no. 3. Cf. footnote 13. The ascending and descending frequencies represent, respectively, judgments of 'heavier' and of 'lighter.'

Series	*E* & *S*	Stimuli 88	92	96	100	104
1	U1	4	10	27	34	84
2		1	11	19	32	88
3		3	8	19	55	93
4		72	41	28	10	1
5		93	49	44	13	1
6	U2	3	18	34	49	77
7		4	7	31	48	95
8		86	67	46	25	15
9		84	71	40	29	1
10		86	63	40	16	2
11	U3	5	14	38	44	87
12		5	10	24	57	88
13		1	3	20	57	89
14		89	72	47	25	4
15		92	80	66	35	5
16	U4	2	8	39	58	90
17		3	21	36	66	88
18		90	79	41	25	6
19		90	65	39	19	4
20		97	86	52	19	5
21	U5	1	14	34	46	70
22		2	17	44	68	93
23		4	17	76	90	98
24		88	71	38	18	6
25		98	78	39	31	3
26	U6	6	13	31	51	82
27		1	2	17	32	60
28		81	62	48	21	4
29		87	82	50	16	4
30		91	71	49	25	12
31	U7	1	8	9	41	65
32		1	4	19	35	79
33		90	70	46	28	10
34		88	83	52	36	6
35		91	69	39	16	2
36	F1	3	26	39	51	85
37		1	23	45	60	79
38		3	5	36	49	67
39		4	12	51	61	77
40		94	58	53	36	10
41		94	69	47	32	12
42		97	84	38	25	15
43		87	81	36	29	10
44		87	75	30	24	11
45	F2	20	32	51	80	79
46		8	27	43	58	68
47		2	14	41	56	79
48		2	16	40	52	74
49		5	27	55	71	88
50		60	34	24	9	9
51		69	32	18	6	3
52		52	25	7	2	2
53		52	15	5	2	1
54	F3	7	30	42	78	83
55		6	31	55	64	77
56		2	7	28	53	74
57		1	3	25	39	61
58		91	55	37	11	13
59		90	55	34	30	14
60		92	80	43	16	7
61		92	75	44	9	5
62		90	81	52	16	4
63	F4	14	41	61	66	78
64		8	43	58	77	82
65		18	28	46	65	73
66		20	27	59	65	69
67		23	28	51	65	74
68		86	58	39	34	22
69		92	57	42	23	18
70		78	65	42	30	25
71		78	73	33	33	29
72		73	67	43	29	20
73	R1	15	33	41	65	62
74		13	31	33	58	77
75		5	22	39	49	69
76		67	61	35	27	14
77		65	44	33	13	8
78		65	52	25	15	13
79		72	52	32	15	7
80	R2	15	19	28	55	78

TABLE I (Continued)

Series	*E* & *S*	Stimuli 88	92	96	100	104	Series	*E* & *S*	Stimuli 88	92	96	100	104
81		15	13	46	60	74	91		68	53	37	22	18
82		12	22	39	56	74	92		75	54	33	16	11
83		14	24	50	62	77	93		75	52	31	20	4
84		69	39	18	8	6	94	R4	11	39	59	83	90
85		75	57	44	17	9	95		8	22	49	57	88
86		72	58	22	9	5	96		11	41	58	67	91
87	R3	14	22	42	44	61	97		13	26	44	72	91
88		13	22	42	52	65	98		83	65	35	18	9
89		16	24	35	59	67	99		90	88	55	16	8
90		62	33	24	18	9	100		84	75	54	5	4

The 100 constant process limens and measures of precisions (L and h) were tabulated to permit comparison with the corresponding limens obtained by interpolation methods.[14] Four interpolation methods were applied to each set of frequencies and so 400 interpolated limens were obtained. All these limens are not published because they have no very direct bearing on the problem.

Two rather familiar assumptions involved in the constant process are as follows: (1) when a liminal or near-liminal stimulus is presented a sufficiently large number of times the frequency of right cases may serve as a measure of the perceptibility of the stimulus; and, (2) the actual relation between percentage of right cases and magnitude of stimulus is a function of the normal curve. Two of our interpolation formulae, which are fundamentally the same, make these same assumptions, and are mathematically sound in the sense that they may be derived from the normal curve. The other two are also similar to each other but they differ from the first pair in that they assume a rectilinear relationship between frequency of correct cases and magnitude of stimulus.

The rectilinear interpolation is simple: given two frequencies of judgment, one less and one greater than frequency 0.5, and given the corresponding stimulus values and their difference, the liminal stimulus is readily obtained by interpolation. Referring to Fig. 1, of rectilinearly distributed frequencies, let:—

R_{50} = limen

R_l = a lower stimulus value, *i.e.* less than R_{50}

R_u = an upper stimulus value, *i.e.* greater than R_{50}

P_l = the difference in the frequencies corresponding to R_l and R_{50}

P_u = the difference in the frequencies corresponding to R_{50} and R_u

$$\text{Then } \frac{P_l}{P_l + P_u} :: \frac{R_{50} - R_l}{R_u - R_l}$$

for corresponding sides of similar triangles are proportional. The only unknown in this proportion is $R_{50} - R_l$ so that is easily obtained, and the value then added to R_l. This result is the liminal value,[15] for:—

$R_l + (R_{50} - R_l) = R_{50}$. The complete interpolation formula is, then:

$$L_{r2} = R_l + \frac{P_l}{P_l + P_u}(R_u - R_l). \qquad [1]$$

[14] The values of the constant process limens of the Fernberger data were taken from his paper, *op. cit.*, 344, 352; but the values of h being unpublished were obtained, by permission of Professor Fernberger, directly from the original records. The other values of L and h, together with the interpolated limens, were computed by the writer or assistants.

[15] Cf. Brown and Thomson, *op. cit.*, 60 f., and Titchener, *op. cit.*, 95 f.

In the symbol 'L_{r2},' L stands for limen, r for the fact that it was obtained by rectilinear interpolation, and 2 indicates that two stimulus values with the corresponding observed frequencies served as the basis for interpolation. The bold-faced figures in Table I are the frequencies to which this formula was applied. In general, those figures were so selected as to include the limen but without further attempt to obtain values most favorable for interpolation.[16] The interpolated limen from the first set of frequencies in Table I is obtained as follows:

$$L_{r2} = 92 + \frac{40}{40 + 34}(104 - 92) = 98.49.$$

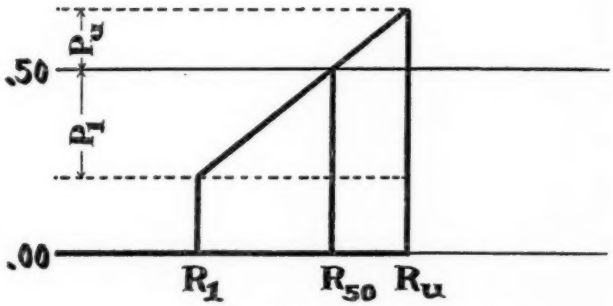

FIG. 1

The symbol "L_{r4}" is used to represent the other variety of rectilinear interpolation, and here the 4 means that four stimulus values with their frequencies were used, instead of two. The means of the two lower stimuli, of the two lower frequencies, of the two upper stimuli, and of the two upper frequencies formed the basis for interpolation:

$$L_{r4} = \frac{R_l + R'_l}{2} + \frac{\frac{P_l + P'_l}{2}}{\frac{P_l + P'_l}{2} + \frac{P_u + P'_u}{2}} \left(\frac{R_u + R'_u}{2} - \frac{R_l + R'_l}{2} \right) \quad [2]$$

The rule for selecting values for this interpolation was to select that set of two adjacent pairs of frequencies the average of whose averages would be nearest 50. The aim here was to obtain optimum values for interpolation whereas for formula [1] we aimed to obtain random values. Using this formula, the limen from the first set of frequencies is obtained as follows:

$$L_{r4} = 94 + \frac{31.5}{31.5 + 9}(102 - 94) = 100.22.$$

[16]The basis of selection of these figures was as follows: the p's corresponding to the stimulus values just above and just below the middle stimulus were used provided the upper p lay within the range + 1.5 sigma and the lower within − 1.5 sigma. In other words, those p-values were used when the lower was between 7 and 50 and the upper between 50 and 93. When these conditions were not fulfilled, one or both p's as the case might be, were selected from among the other p's in order to obtain the two p's within the selected ranges. In this way, then, the extremes of the distribution were avoided and p-values on either side of the limen were chosen; but there was no further attempt to select the most favorable figures for interpolation.

It will be observed that L_{T4} is based upon twice as many observations (400 in the present data) as L_{T2}. In other words, L_{T4} is based upon four-fifths of all the data and L_{T2} upon two-fifths.

The formulae for curvilinear interpolation differ from those just discussed only in that they assume normality of distribution.

$$L_{c2} = R_l + \frac{V_l}{V_l + V_u}(R_u - R_l) \qquad [3]$$

The symbol 'V_l' stands for the distance in terms of P.E. (or gamma or sigma)corresponding to the difference in the frequencies for R_l and R_{50}, or expressed more briefly, to P_l. The value of V_l is immediately found by entering a table of the normal probability integral with the value of P_l. Similarly V_u corresponds to P_u. The values of V_l and V_u may be obtained in terms of P.E., gamma, or other dispersion unit indifferently, for it is only the ratio $\frac{V_l}{V_l + V_u}$ which the formula requires. Except for these substitutions for P_l and P_u, this formula [3] is identical with [1]. Furthermore, formula [4] is analogous to [2]:

$$L_{c4} = \frac{R_l + R_l}{2} + \frac{V_l}{V_l + V_u}\left(\frac{R_u + R'_u}{2} - \frac{R_l + R'_l}{2}\right) \qquad [4]$$

The 'L_{c4}' limen from the first set of frequencies:

$$L_{c4} = 94 + \frac{.8965}{.8965 + .2275}(102 - 94) = 100.38$$

It was found necessary to extrapolate eight of the 400 limens for in those cases the liminal values fell outside the selected frequencies. This extrapolation was performed with appropriate modifications of the formulae given above.

Several points concerning these formulae may now be summarized: (1) the first two make the assumption of rectilinearity; (2) none of them employs all the data; (3) none of them takes account of the Müller-Urban weights; (4) all of them are very easy to apply; (5) the accuracy of any of them, as compared with the constant process, depends in some measure upon the symmetry of placement of the selected frequencies; (6) the accuracy of the last two also depends upon the normality of the data; (7) none of them can be expected to give results consistently identical with those of the constant process. Our specific problem, to repeat, is to discover the degree to which these interpolated values do approximate the constant process values, in data chosen from the literature.

Results

In Table II are the Pearsonian coefficients of correlation between the constant process and the several interpolated limens; they are all positive and very high with P. E.'s very low. The left pair of coefficients in the table, moreover, are almost identical and somewhat lower than the right pair, which are identical. Thus it comes about that the probable correctness of the direction of difference between either L_2 coefficient and either L_4 coefficient, is very nearly 1.000. On the basis of these results, then, the L_{T4} and L_{c4} interpolations appear significantly

superior to the others; while at the same time all four appear closely to approximate the constant process. This, however, does not throw much light on the interesting problem of the reliability of the single interpolated limen. In the bottom row of the table are given the probable errors of estimation of constant process limens from interpolated limens. They indicate the probable deviation, in terms of grams, of an estimated or interpolated limen from the constant process value. It may be remarked that this deviation for the L_{r_4} and L_{c_4} interpolations is of the order of only 0.3 grm.

Another comparison was made between the constant process and interpolation methods by computing the P.E. of each constant process limen and then obtaining the departures, in terms of these P.E.'s, of the limens interpolated from the constant process. Boring's formula for $P.E._L$, not to be confused with that for $P.E._{av}$, was used.[17] The actual gram differences between the interpolated and constant process limens were first obtained and then converted into $D/P.E._L$. Fig. 2 exhibits the results graphically. There is a separate histogram of the departures of each of the four kinds of interpolated limens. The abscissa unit is 0.1 $P.E._L$ while the ordinates are frequencies of departures. It may be noted that the L_{r_4} and L_{c_4} limens depart least from the constant process. The L_{c_4} distribution, furthermore, exhibits the greatest normality and the least variability, as well as equal numbers of positive and negative divergencies.

TABLE II

CORRELATIONS BETWEEN CONSTANT PROCESS AND INTERPOLATED LIMENS

	L_{r_2}	L_{c_2}	L_{r_4}	L_{c_4}
r	.962	.960	.987	.987
$P.E._r$	.0050	.0053	.0017	.0017
$P.E._{est.L}$	.4992	.5382	.3328	.3263

Frequencies of divergencies falling between various P.E. limits are shown in Table III. Since there are 100 of each variety of limen these figures are percentages as well as actual cases.

The values in Table IV below are also in terms of $P.E._L$ and allow of some comparison between the variability of the deviates and that of the constant process. Q has been taken as the measure of the variability of the deviates because, practically, we are interested in the variability of the single limen. As might be expected, the medians fall close to zero deviation from the constant process.

[17] E. G. Boring, this JOURNAL, 27, 1916, 317.

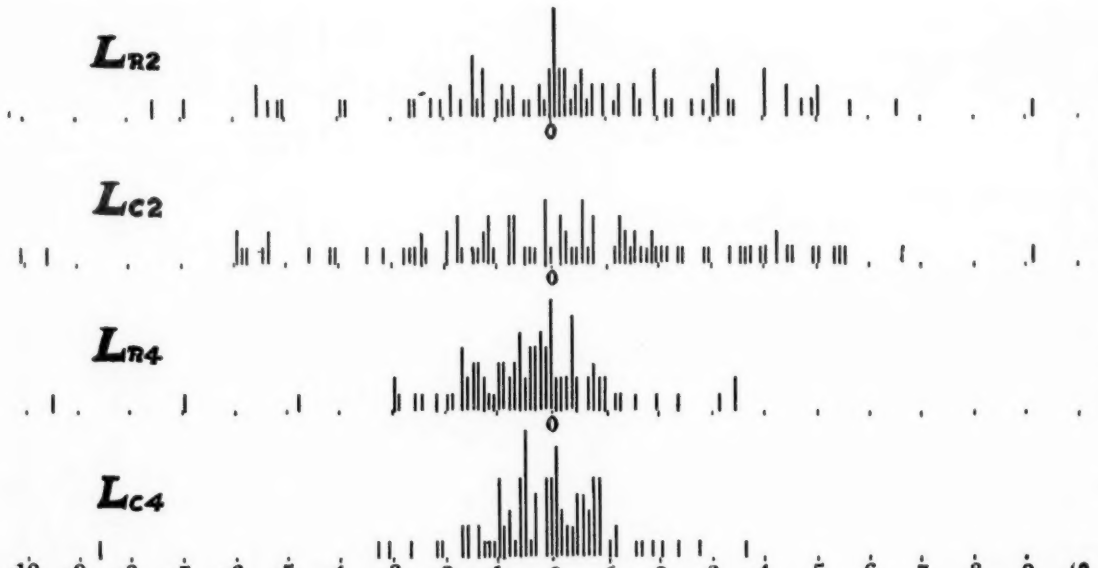

FIG. 2. DEPARTURES OF INTERPOLATED LIMENS FROM THE CONSTANT PROCESS IN TERMS OF P.E. OF THE LATTER.

TABLE III

FREQUENCIES OF VARIOUS DEVIATIONS FROM THE CONSTANT PROCESS

P.E.$_L$	L_{T_2}	L_{C_2}	L_{T_4}	L_{C_4}
within ± 0.5	24	15	40	41
1.0	38	29	65	74
1.5	49	44	77	83
2.0	58	57	87	90
2.5	62	64	90	94
3.0	66	69	94	96
3.5	76	72	97	98
4.0	78	76		99
4.5	85	83		
5.0	87	85	98	
5.5	92	91		
6.0	95	96		
6.5				
7.0	97	97	99	
7.5				
8.0	98			
8.5				
9.0				100
9.5	99	98	100	
10.0		99		
10.5	100	100		

TABLE IV

THE QUARTILES AND SEMI-INTERQUARTILE RANGES OF DEVIATIONS FROM THE CONSTANT LIMEN.

	L_{T_2}	L_{C_2}	L_{T_4}	L_{C_4}
Q_1	−1.43	−1.70	−1.20	−0.73
Median	0.09	0.17	−0.28	0.00
Q_3	2.03	1.85	0.30	0.55
Q	1.73	1.78	0.75	0.64

Both Tables III and IV show interesting differences in the dispersion of the four kinds of deviates. As noted above, the distributions L_{T_4} and L_{C_4} exhibit least dispersion, L_{C_4} being somewhat less dispersed and more symmetrical than L_{T_4}.

It seemed desirable to get some reply to one other question which would have value as a fairly general check on the other results. How do the differences between the constant process limens of the same *S*, obtained at different times to be sure but otherwise under fairly comparable conditions, compare with the corresponding departures of the interpolated limens. We applied this question to one or two pairs of constant process limens of every one of the 15 *S*s. The comparison is shown in Table V by means of 38 ratios for each of the four interpolation methods, and aims to be fairly representative of all the data. Column 1 refers to the observed frequencies given in Table I. Column 2 gives the gram differences between the members of pairs of the constant process limens, each pair being of a particular *S* and both members being either upper or lower limens. The difference $(L' - L'')$ for the first pair of constant process limens, *e.g.* is 2.44 grm. The remaining columns contain the

TABLE V

DIFFERENCES BETWEEN CONSTANT PROCESS LIMENS OF THE SAME *S* COMPARED WITH CORRESPONDING DEPARTURES OF INTERPOLATED LIMENS

*O*s Freqs. cf. Table I	L′ − L″ in grams	D of L_{T2} L′ − L″	D of L_{C2} L′ − L″	D of L_{T4} L′ − L″	D of L_{C4} L′ − L″
4	2.44	.34	.13	.02	.07
5	"	.23	.42	.03	.03
6	.51	1.47	1.22	.14	.04
7	"	1.75	2.12	.06	.22
14	1.75	.18	.16	.03	.13
15	"	.09	.18	.36	.25
16	.67	.76	1.18	.01	.04
17	"	.57	.36	.01	.04
24	1.24	.33	.22	.15	.06
25	"	.62	.70	.21	.02
26	3.46	.22	.23	.00	.03
27	"	.10	.05	.11	.05
34	2.01	.49	.59	.10	.18
35	"	.12	.02	.04	.03
36	.21	7.72	7.81	.57	.57
37	"	.05	.52	1.14	1.29
38	2.16	.04	1.67	.07	.08
39	"	.50	.52	.28	.32
50	.91	1.17	.68	.09	.04
51	"	.86	.67	.11	.13
52	.41	1.32	1.10	1.12	1.24
53	"	1.98	1.88	4.40	.32
54	.36	3.25	3.50	.06	.39
55	"	.69	.61	2.28	2.47
56	1.91	.19	.13	.11	.00
57	"	.11	.06	.12	.12
69	.32	4.56	4.91	1.38	1.16
70	"	.00	.13	1.81	1.63
71	.89	.88	.94	1.15	1.04
72	"	.73	.69	.25	.20
73	.23	8.80	8.75	1.57	1.57
74	"	1.83	1.57	.04	.04
85	1.45	.12	.23	.02	.00
86	"	.81	.63	.20	.04
88	.47	.06	.15	.02	.09
89	"	2.19	1.96	.15	.04
98	2.03	.18	.09	.07	.07
99	"	.00	.06	.34	.20
Per cent less than L′ − L″		71	68	79	82

ratios of the gram deviation of the interpolated limen (D) to the gram difference between the two constant process limens ($L' - L''$). The 0.34 at the top of column 3, *e.g.* means that the L_{T2} interpolated limen departs from the constant process limen (both based on the No. 4 set of observed frequencies) only .34 as much as the two constant process limens (based respectively on No. 4 and No. 5 sets of observed frequencies) depart from each other. The second figure in column 3, viz., 0.23, is to be similarly interpreted: it refers to the interpolational deviation from the constant process limen based on observed frequencies No. 5 *i.e.* on the second member of the first pair of observed frequencies. These ratios, then, show how much greater or less than $L' - L''$ the deviations of the interpolated limens are.[18]

The columns of this table are not averaged partly because they are, and properly, not normal. The figures of the bottom row, however, are the relative frequencies with which the interpolational deviates are less in value than the difference between the two constant process limens of the given *S*. The first figure in the bottom row, for instance, means that 71% of the L_{T2} limens departed less from the constant process than did two constant process limens from each other, even though the latter were of the same *S* under similar environmental conditions. The mean of the entire bottom row is 75, which means that 75% of all the departures are less than $L' - L''$, the difference between the constant process limens. The mean for the L_{T2} and L_{C2} values is 69.5 as compared with 80.5 for L_{T4} and L_{C4}.

The logic of Table V is this: L' and L'' are supposed to be comparable for they are supposed to be representative for the subject and to differ because of minor 'chance' changes in conditions. The difference between them should not be significant. Since the difference $L' - L''$ ought not to be significant, then those deviations of interpolated limens from constant process limens which are of the same order or less than $L' - L''$, ought not to be significant. As mentioned in the preceding paragraph,

[18]Culler has recently recommended a new formula for P.E.L (*Psychol. Monog.*, 35, No. 2, 1926, 87 f.) which had not been available to the writer during the course of this investigation. Quite aside from the question of the relative merit of different formulae, it may be remarked that, when the P.E.'s of the constant process limens are recalculated with this formula, the values are roughly 2.4 times as great as with the formula suggested by Boring in 1916. The ratio of the results of the two methods does, indeed, make some approach to uniformity. Thus, when the P.E. of the one method is divided by the P.E. of the other, the smallest quotient in the writer's 100 instances is approximately 2.0 and the largest 2.7, while the mean ratio is 2.39 with 0.12 as P.E. of the *single* ratio. Therefore, in the present data, the order of magnitude of the one P.E. usually can be obtained from the other by simply multiplying or dividing by the constant 2.4. Of course, as regards the present investigation, P.E.'s of such increased magnitude may substantially increase the conservatism of the principal conclusions. Referring to the L_{C4} distribution in Fig. II, for instance, about 94% of the interpolated limens would fall within ± 1 P.E. of Culler's as compared with the 74% indicated in the figure (or in Table III).

75% of all the interpolated limens were found to deviate from the constant process by less than the amount $L' - L''$. This result seems remarkable, especially when one recalls that the interpolation was performed without any weighting.

There are certain objections which might be raised against the procedure underlying Table V. No account is taken of differences in variability; the actual gram differences only are considered. Then, too, it should be recalled that about 60% of the observed frequencies given in Table I were obtained by summing the results of two sets of 50 series each.[19] These sets of 50 were presumably obtained under comparable conditions, but, even so, summing them might increase the difference between constant process limens figured from them. That would result in a relative decrease in the departures of the interpolated limens. Since the other 40% of observed frequencies in Table I were not obtained in this way, however, they serve as a check upon this sort of error. The ratios in Table V corresponding to these latter 40%, *i.e.* to Nos. 36-72, do in fact run somewhat larger than the others, a condition which, while it tends to support the objection mentioned, might well be due nevertheless to sampling variations.

Summary

(1) Limens (L_{r_3}, L_{c_2}) interpolated from two-fifths of data regarded adequate to the constant process, and when little attempt is made to secure those portions of the data which would yield the most accurate limen, often depart widely from the constant process.

(2) Limens (L_{r_4}, L_{c_4}) interpolated from four-fifths of the data, and when some attempt is made to secure the most favorable four-fifths, usually approximate quite closely the constant process.

(3) Interpolation (L_{c_4}) assuming normality and utilizing four-fifths of the data appears the most accurate of all the tested methods; in other words the writer's results favor the use of formula [4].

(4) If these results are typical of data from lifted weight and other experiments, it would seem that the labor of computing constant process limens, reduced though it certainly has been, is quite unjustified, at least for many situations.

These provisional results suggest other problems which seem never to have been solved empirically. It seems theoretically possible, for instance, that the number of the stimuli might legitimately be still further reduced from five to four, three or even the traditional two. If such reduction is legitimate, the experimentation (as well as the computation) for a limen could be substantially reduced. The requisite number of series and the changed attitude of the subject with the reduced stimuli, would undoubtedly be important considerations. The present study was designed in part to give preliminary information on this question, *i.e.* to indicate whether an experimental investigation of it would be likely to prove worthwhile. Since the L_2 methods were applied to observed frequencies taken fairly at random within a wide range, we might expect

[19] Cf. footnote 13.

that actual experimentation with only two comparison stimuli would yield comparable frequencies within that range. But considerable preliminary work as well as numerous extra trials during the experiment over a distinctly wider range than that of the two selected stimuli, might well be found necessary; the former to assure the selection of two suitable stimulus values, and the latter as *Vexierversuche* as well as to provide some extreme and hence easy judgments to give the subject confidence and orientation. Yet even assuming no difficulty along such lines the preliminary indication is negative; the use of two comparison stimuli with 100 series would probably be inadequate, for the interpolations based upon them were found inadequate when five comparison stimuli were actually employed experimentally. This does not imply, of course, that if those two frequencies most favorable for interpolation were selected from five observed, that the result would be unsatisfactory.

The L_4 methods were applied to 'selected' data, but really there was not in these cases much opportunity for selection (four adjacent observed frequencies were selected out of the five), and also the data varied widely with respect to normality and precision. The writer feels, therefore, that the results obtained from the L_4 methods give a useful preliminary indication of what to expect from the experimental use of two comparison stimuli with 200 series; the indication is that the investigation would prove worthwhile, and the results interesting, in any event. The L_4 methods obviously give favorable indications with respect to the use of four comparison stimuli with 100 series. The whole question is one which demands actual experimentation with different numbers of comparison stimuli. The results, as do ours, would have a bearing upon the applicability of tables like Fullerton and Cattell's.[20]

A quite different problem is that of an adequate measure of variability of an interpolated limen.[21] Computing the P.E. on the basis of the data used in interpolating the limen, would be easy and possibly adequate.

There are a number of shorter methods for computing limens which might be compared with the constant process, for instance, Wirth's[22] and Spearman's[23] methods and the quartile method mentioned by Brown and Thomson.[24] Applying such methods to the observed frequencies after the latter had been simply weighted might increase the value of the results.

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[20]A. S. Fullerton and J. McK. Cattell, *On the Perception of Small Differenc es* (Publi a ctions of the U. of Penn., Phil. Series, No. 2), 1892, 16.

[21]Brown and Thomson, *op. cit.*, 60 f.

[22]R. Pauli, *loc. cit.*

[23]C. Spearman, *loc. cit.*

[24]Brown and Thomson, *op. cit.*, 60 f.

FOOD *VS.* SEX AS INCENTIVES FOR MALE RATS ON THE MAZE-LEARNING PROBLEM[1]

By Calvin P. Stone and Mary Sturman-Huble, Stanford University

No doubt every one who has handled rats on a maze problem has wished for an experimental technique that would effectively impell them to disregard irrelevant objects on the floor, the alley walls, or maze covering and to attend more strictly to the business of learning and running the maze. Hunger and the food-reward are serviceable activators of movement, yet one cannot overlook the fact that rats in a state of hunger are frequently distracted by irrelevant stimuli in the maze. Because of this the reliability of one's criteria of learning is considerably lowered.

In this preliminary report our interest centers chiefly in determining whether sex-hunger and an opportunity to copulate at the end of each run will cause the rat to attend more strictly to the business of learning and rapidly tredding the maze than a state of food-hunger and the opportunity to appease this appetite.

Animals. Albino rats were used because previous experiments[2] assured us that an adequate supply of receptive females could be made available for each day's work. This can be assured for the mouse and rabbit, as well, but for our purpose the rat is preferable because more is known of his maze performances than of either of the other animals. Food and sex groups consisted of 9 animals each.

The following brief report deals with animals approximately one year old. Preliminary experiments on males of other ages, however, have been run. Before being placed on the experiment each male was tested for copulatory ability with a receptive female, and at irregular intervals prior thereto had been given opportunities to copulate many times. Thus for sex as well as for food the animals demonstrated the presence of appetite and the ability to satisfy it when opportunity was afforded.

Diet. It is of paramount importance in an experiment of this kind to have a well balanced diet not only for the males of the experiment but also for the colony of females from which each day's quota of receptive females

McCollum Diet

Whole wheat ground fine	67.5
Casein	15.0
Powdered whole milk	10.0
Calcium carbonate	1.5
Sodium chloride	1.0
Butter fat	5.0
	100.0

[1]Financial support for this study was granted by the National Research Council through the Committee for Research on Sex Problems.

[2]C. P. Stone, The awakening of copulatory ability in the male albino rat, *Amer. J. Physiol.*, 68, 1924, 407-424; Delay in the awakening of copulatory ability in the male albino rat incurred by defective diets: I. Quantitative deficiency, *ibid.*, 69, 1924, 195-224.

is selected in order that sexual aggressiveness and sexual receptivity, respectively, will be at high level. Our diet consisted of the McCollum mixture which has proven eminently satisfactory in other experiments on sexual behavior in our laboratory.[3]

Mazes. The animals were run on two types of mazes. (1) Maze A is a simple T-maze allowing only one turn from the starting pathway either to the right or to the left. The distance from start to exit is 48 in. (Measurements taken down the center of the alleys with right-angled turns.) The simplicity of this maze permits the male to reach the reward in 2 or 3 sec. after he has learned to go to the proper side. Since each trial involves a choice of right or left turn, however, several trials must be run before the male learns which turn to take. (2) Maze B is a close approximation of a maze described by Warden.[4] In this maze the distance from start to exit, as measured along the true pathway, is 319 in. The home box was used in our experiments only as a vestibule leading to a portable cage placed just outside of the maze. In the portable cage food or the receptive female was kept. This maze is one of only medium difficulty for rats aged 30 days or over and can be traversed by large animals in a minimum of 5 or 6 sec. In mazes of this type the errors in learning are by no means directly proportional to the number of blind alleys, as is more nearly the case in a multiple T-maze in which a definite choice is involved at each unit of the maze.

Trials. Every animal was given 1 trial per day on the simple maze until 15 trials had been completed. Immediately following this he was given 1 trial daily on the more complex maze until 15 trials had been run.

Rewards. Individuals of the food group were given an opportunity to eat for a period of 20 min. in their portable cages set aside at the end of each run. With Maze A the food thus consumed was the daily ration. Toward the middle of the experiment with Maze B, however, an addition of 2 grm. per animal was alloted in the home cages at the end of each day's work. This was found necessary to offset a small but steady drop in the weight of the food group after the early days of the experiment. Approximately 30 min. after the individuals of the food group were returned to the home cage, a receptive female was introduced into their cages to permit each male to copulate 3 or 4 times and thus, on the physiological side, balance the daily sexual experience of the sex group. This sex activity in the colony room, being separated in time by a half hour or more from the runs on the maze, we believed would not react on the learning process as an additional incentive for running the maze. In one other preliminary experiment with 8 animals this copulatory experience in the home cages was not permitted the food group. Their results do not appear to be significantly different from those of this experiment.

Every animal of the sex group had an excess of food in its home cage throughout the experiment and could eat *ad libitum* according to the dictates of appetite. After completing the run in the maze he was permitted to copulate from 3 to 5 times in the portable cage. He was then returned to the home cage, there to remain until the next day.

From the foregoing description it may be inferred that our experimental technique, so far as handling incentives is concerned, is more complicated than that used by Moss,[5] Simmons,[6] or Tsai.[7] Every male of the food and sex groups was required to demonstrate sexual aggressiveness by the daily

[3] C. P. Stone, *ibid.*, 68, 1924, 407-424.

[4] C. J. Warden, The distribution of practice in animal learning, *Comp. Psychol. Monog.*, 1, 1923, No. 3, 1-64.

[5] F. A. Moss, Study of animal drives, *J. Exper. Psychol.*, 7, 1924, 165-185.

[6] R. Simmons, Relative effectiveness of certain incentives in animal learning, *Comp. Psychol. Monog.*, 2, 1924, No. 7, 1-79.

[7] Chino Tsai, The relative strength of sex and hunger motives in the albino rat, *J. Comp. Psychol.*, 5, 1925, 407-415.

copulatory test. Whatever deleterious influence, if any, might arise and be reflected in the activity of the sex group as a result of daily sex activity, would also affect the food group which had in its home cage an equal amount of sexual activity. Both groups were kept at a high nutritional level, as our weight records will show, to insure that there would not be a

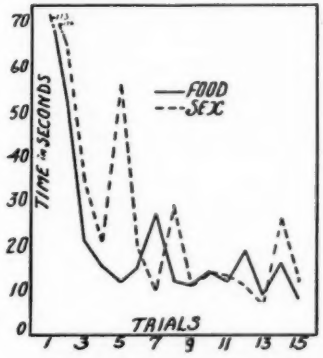

FIG. 1. Median time for daily runs by food and sex groups on T-maze.

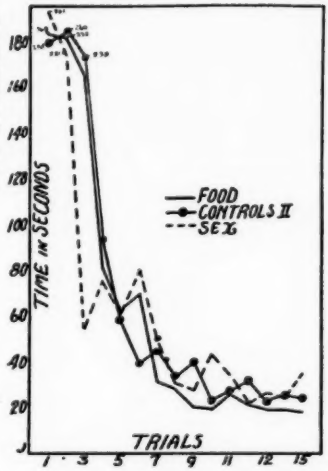

FIG. 3. Median time for daily runs on complex maze. Controls II is a second food group with weights reduced below the regular food group of the experiment.

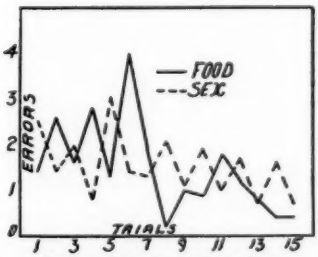

FIG. 2. Mean number of errors for daily runs on T-maze.

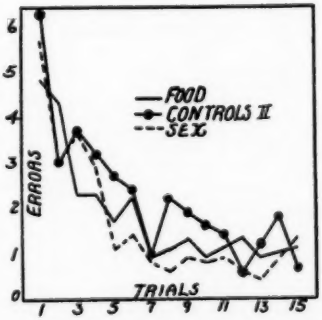

FIG. 4. Mean number of errors for daily runs on complex maze.

waning of the sexual drive as a result of chronic inanition. Only a small number of copulations per day was allowed each animal in order that we might be reasonably sure, in the light of previous experiments in our laboratory, that potency would not disappear toward the end of the experiment.

Criteria of Learning. The time records consist of the total time from the rat's initial start until his nose reached the doorway of the home-box. Although the records do not include delays in leaving the starting position or hesitation in entering the home box at the end of the runs, static time was not removed.

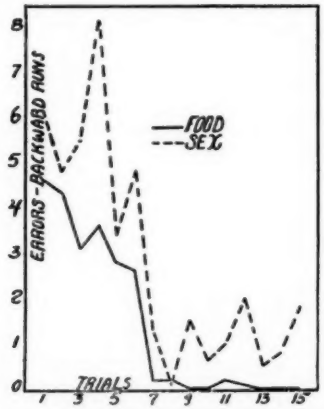

FIG. 5. Mean number of pathways retraced in daily runs on complex maze.

An error consists of any deviation from the true pathway, *i.e.* an entrance into a blind alley as the animal was going away from or back to the starting point. To commit an error the animal must progress beyond the entrance to the alley with two-thirds or more of his body length, tail excluded. Backward turns on the true pathway are included in the error records of Maze A, but not in Maze B. For the latter a separate record of the pathways traversed on backward runs was made.

RESULTS

Maze A (T-Maze). The data for the T-maze are summarized in Table I. Because certain animals of each group made erratic runs from day to day the means are less adequate representative of the average time per trial than the medians. The latter were selected for the learning curves of Fig. 1. As will be observed upon considering the graphs of Fig. 1, the general trend of the time curves for the food and sex group is quite similar. Owing to the small number of cases, however, it is impossible to go beyond rough comparisons of the general trends of these curves. Fig. 2 gives the mean error curves for daily runs. Here although the curves are quite similar, there appears to be a very slight superiority on the part of the food group in learning the

correct route. The median number of errors for the food group is consistently lower than that of the sex group as may be seen from Table I. Whether the incentive is responsible for this slight superiority in learning by the food group or whether there is a slight intellectual superiority of this group over the sex group we cannot say. However, the probability of the latter being the case is somewhat increased from a consideration of the results of experiment 2, in which the food group became the sex group and the sex group the food group.

Maze B (*Complex Maze*). The summarized data of experiment 2 are given in Table II. One animal of the sex group was excessively slow throughout the 15 trials, and other animals of both groups made excessively slow runs from time to time, hence the mean time for daily runs is again less adequate than the medians for purposes of comparison. Fig. 3 graphically presents the trends in time of successive runs for the two groups. As will be seen the curve for the food group drops to a slightly lower level than that of the sex group. Individual animals of the latter, however, were equal to the best of the food group.

As to errors, the situation for the two groups is the reverse of what was found for time although the difference is very slight and probably insignificant (Fig. 4). In the previous experiment, it will be recalled, these same animals were slightly superior in the reduction of errors, although at that time food was the incentive.

The records for retracing show that there was very little difference between the two groups as a whole. One animal of the sex group was an inveterate retracer, however, and his record makes the mean number of alleys retraced by the sex group slightly higher than the mean of the food group (Fig. 5).

Weight Records. In Tables I and II mean weights for the two groups of this experiment are given for each day. Inspection of these data show that the sex group, which was allowed to feed in the home cages according to the dictates of appetite, varied little from day to day. The food group, however, continually lost a little weight due to the fact that they did not consume sufficient food in the 20-min. feeding period to hold them at the high weight level maintained under conditions of unrestricted feeding. Their physical condition, nevertheless, was excellent.

In our preliminary experiments on this problem we foresaw the desirability of inquiring into the relation of speed of running by very heavy animals as compared with similar animals whose weight was quickly dropped at the beginning of the experiment from 15 to 20 percent and thereafter maintained at this lower level. A preliminary experiment on this point was made. Ten

one year old males starting with a mean weight of 275 grm. were dropped during the trials preliminary to Maze B to a level of 220 grm. where by careful dispensation of food to each animal according to his daily weight he was held practically at maintenance throughout the experiment. Their time and error records are graphically represented (Controls II) in Fig. 3 and 4. It was found that after the very erratic first trials very little difference in the time and error curves appeared. These slight differences are probably insignificant.

Conclusions

Considering all criteria together it would seem that one is justified in drawing the tentative conclusion that food and sex as used in this experiment are approximately equal as incentives

Table I
Summary of Data for Maze A (T-Maze)

Criteria of Comparison	Group	Trials														
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Mean Time per run	Food	121	101	56	55	86	42	15	31	33	58	66	8	25	17	23
	Sex	124	64	56	26	83	37	34	53	16	35	20	65	15	25	15
Median Time per run	Food	173	52	21	15	12	15	27	12	11	14	12	19	9	16	8
	Sex	126	64	34	21	56	20	10	29	11	14	13	11	7	26	12
Mean number Errors	Food	1.4	2.6	1.6	2.8	1.3	4.0	1.9	.2	1.0	.9	1.8	1.4	0	.4	.4
	Sex	2.5	1.4	2.0	.8	3.1	1.4	1.3	2.1	1.1	1.9	1.0	1.7	.7	1.6	.7
Median number errors	Food	2	1	0	0	1	0	1	0	0	0	0	0	0	0	0
	Sex	2.5	1	2	1	3	1	0	2	0	2	1	0	0	1	0
Mean weights	Food	300	287	289	285	282	281	282	275	277	277	278	275	275	267	270
	Sex	309	298	300	294	293	288	292	285	292	288	290	289	291	286	286

Table II
Summary of Data for Maze B (Complex Maze)

Criteria of Comparison	Group	Trials														
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Mean trial per run	Food	640	536	197	367	240	113	35	32	25	22	26	20	22	20	18
	Sex	819	80	498	411	122	169	54	35	63	86	59	132	58	64	119
Median time per run	Food	300	253	166	81	63	70	31	28	20	19	26	21	19	19	18
	Sex	801	175	54	75	61	80.5	50	30.5	27	43	34.5	21.5	26.5	24	34.5
Mean number errors per run	Food	4.8	4.3	2.3	2.3	1.7	2.2	.9	1.1	1.3	.9	1.1	1.3	.9	1.0	1.1
	Sex	5.7	3.0	3.6	2.9	1.1	1.4	.8	.6	.9	.8	.9	.6	.4	.9	1.3
Mean number alleys retraced on true pathway	Food	4.6	4.3	3.1	3.6	2.8	2.6	.2	.2	0	0	.2	.11	0	0	0
	Sex	6.1	4.8	5.5	8.1	3.4	4.8	1.3	.1	1.5	.6	1.0	2.0	.5	.8	1.8
Mean body weight	Food	290	280	278	275	270	264	258	258	260	251	253	250	252	251	250
	Sex	299	300	294	299	292	288	298	299	303	297	294	297	302	295	297

to maze learning and running by one year old male rats. As everyone knows, however, mathematical comparison leading to more exact conclusions demand that a number greater than 18 individuals be submitted to the experiment. This we expect to do when a maze of *high reliability* with which we are now experimenting has been satisfactorily proven.

INTERESTS OF COLLEGE STUDENTS[1]

By M. O. Wilson, University of Oklahoma

The Problem

It is the main purpose of this study to analyze the interests of college students, as indicated by their selection of courses and their achievement in these courses, in order to characterize, if possible, certain types of students with respect to their chosen fields of intellectual activity. One of the most important problems of undergraduate education is to fulfill the needs of students with respect to their different abilities and interests and to turn out men and women who are prepared to meet the various needs of society. Therefore, such a study as the one here described should be of some practical value to the maker of the college curriculum, the college administrator, the vocational guidance expert, and the teacher, in so far as the interests of college students thus expressed reveal the needs of the students themselves and of society in general.

Previous Investigations

One of the earliest studies dealing with the phase of the problem outlined above was that of Strayer and Thorndike.[2] They made a statistical analysis of the courses actually taken for the A.B. degree by 391 men students graduated in 1909 at different American colleges where various degrees of freedom in selection of courses were permitted. Among other tendencies, their results show (1) that specialization toward a profession will occur when it is permitted, (2) that free election (but within non-professional courses) increases specialization outside of languages and literature, and (3) that in certain colleges specialization by the candidates for the A.B. degree is chiefly in languages and literature, a specialization artificially cultivated by the requirements for entrance and graduation.

In determining the relation between courses taken and achievement on the one hand and securing the *cum laude* degree in the professional schools on the other, Lowell in 1911 found a tendency for the future *cum laude* men to take more courses in the classics and mathematics than the "plain" men, while the latter showed a stronger tendency to select subjects reputed to be less difficult.[3]

In 1912, Thorndike selected some 300 college junior students and had them rate their own interests in certain school subjects, first at the elementary school level and again at the high school level.[4] He then correlated their rankings with their achievement and secured a coefficient of 0.91. From this result he concluded that a person's relative interests are quite permanent and that they are "extraordinarily accurate symptoms of his relative capacities." This study was duplicated by Bridges and

[1]This article is part of a Ph.D. dissertation submitted by the writer to the Department of Psychology, the University of Chicago. The study was carried out under the direction of Professor L. L. Thurstone, to whom many obligations are gratefully acknowledged.

[2]G. D. Strayer and E. L. Thorndike, *Educational Administration*, Section 17, 1913.

[3]A. L. Lowell, College students and professional training, *Educ. Rev.*, 42, 1911, 217-223.

[4]E. L. Thorndike, Permanence of interests and their relatiom to abilities, *Pop. Sci. Monthly*, 81, 1912, 449-456.

Dollinger, in which they found the correlation to be much less.[5] They took issue with Thorndike as to the relation between capacities and interests. In 1917, Thorndike reported an extension of this study in which he confirmed the previous results.[6]

In 1917 Davis investigated the question as to what were the courses pursued by members of Phi Beta Kappa.[7] He found that at the time the study was made and for the group concerned, English, classics, and mathematics still constituted approximately two-thirds of the material pursued by high school pupils whose intellectual superiority was later evidenced by election to Phi Beta Kappa. Further, when these students reached college, there was a tendency to substitute modern languages for the classics, and the social sciences assumed leadership in the list of subjects taken.

In the same year King made a study similar to that of Thorndike, based on 140 college students.[8] The correlation between achievement in college and interest in college subjects, as indicated by the students' own rankings, was 0.73. But, when this correlation was computed for interest and achievement throughout the elementary, high school, and college courses, the coefficient dropped to 0.47.

In 1923, Thorndike had his advanced students in education, mainly teachers and administrators, rank the high school subjects according to their interests in them.[9] He found that English, literature, history, and science were the most liked, and Latin the least. The men favored history and science, and the women literature.

Stoy made a study in 1925 of the relation between preferences of freshman students and scores made on certain psychological entrance examinations.[10] He found that mathematics and exact science students excelled in the quantitative tests, such as arithmetic and estimating, and the exact science students did poorly in the artificial language tests. The foreign language students and literature students excelled in opposites and grammar. The foreign language group excelled also in artificial language and the literature group excelled in proverbs, but did poorly in arithmetic. The history group excelled in no particular tests but did poorly in arithmetic, artificial language, proverbs, grammar, and reasoning.

Method

The Criteria of Interest. For the purpose of this study two criteria of interest are set up, namely, selection of courses and relative achievement. It is assumed, according to the first of these, that when a student chooses a special field of intellectual activity in which to make an intensive study, this is a strong indication of his desires and needs as he senses them at the time. It is further assumed that on the whole the selection of courses in other fields for the purpose of getting some "acquaintance with the intellectual wealth of the world and with the main human problems of the present day" is also an expression of interest in so far as prevailing administrative regulations permit free election.

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[5] J. W. Bridges and Verona Dollinger, The correlation between interests and abilities in college courses, *Psychol. Rev.*, 27, 1920, 308-314.

[6] E. L. Thorndike, Early interests: their permanence and relation to abilities, *School & Soc.*, 5, 1917, 178-179.

[7] C. O. Davis, Courses pursued by members of Phi Beta Kappa, *School & Soc.*, 5, 1917, 686-690.

[8] I. King, Permanence of interests and their relation to ability, *School & Soc.*, 6, 1917, 359-360.

[9] E. L. Thorndike, The interests of teachers in studies during their high school course, *School & Soc.*, 17, 1923, 419-420.

[10] E. G. Stoy, The relationship of interests in high school subjects and ability in certain psychological tests. An unpublished M. A. Thesis. Typewritten copy in the University of Chicago Library, 1925.

The second criterion assumes that in general the student will put forth his best efforts in those subjects in which he is most interested. It is granted that the exceptions to this assumption are quite numerous, but it is believed to be true in a broad sense. It seems to be justified by most of the investigations referred to in the previous section.

In order to determine further the validity of this assumption an attempt was made to secure additional information concerning the "theory of two factors of intelligence" as supported by Spearman. If, as Spearman maintains, there is a common fundamental function or group of functions underlying the various intellectual activities of students, then, the validity of the achievement criterion is strengthened. But, if, as Thorndike maintains, intelligence is made up of specific abilities, then, this must be allowed for in the use of this criterion. Two methods of attacking this problem were used.

(1) For the first method, the coefficients of correlation between achievement of the students in the various departments were obtained. These coefficients were then treated statistically according to a procedure outlined by Spearman and Holzinger, which is somewhat as follows.[11] The criterion for determining the extent to which a set of data conform to the general ability theory is expressed by the relation

$$\frac{r_{13}}{r_{14}} = \frac{r_{23}}{r_{24}} \qquad [1]$$

where the subscripts represent any four of the abilities in question and the r's the coefficients of correlation between them. Then, for the true correlation or those derived from some 'entire population' the relation may be expressed thus:

$$F = r_{13}r_{24} = r_{23}r_{14} = 0. \qquad [2]$$

Now, if the 'entire population' is replaced by a sample and several abilities are correlated, we have

$$\text{Mean } F = 0 \ (\pm \text{ the error of sampling}) \qquad [3]$$

The formula for finding the error of sampling is:

$$\sigma_F = 2\sqrt{\frac{\bar{r}^2(1-\bar{r})^2}{N} + \frac{S^2}{N}} \qquad [4]$$

where S = standard deviation of all r's, $\bar{r}$ = mean of all r's, and N = population. Applying these formulae to the correlational coefficients of this study the following results were obtained as compared with the theoretical values needed to bear out the hypothesis:

Status of Value	Mean of all F's	Standard Deviation of all F's	Percentage of F's falling within $\pm$ 1 Theoretical S. D.
Theoretical	0.0000	0.0327	68.26
Observed	0.0043	0.0266	70.00

Considering the closeness with which these three values—the mean, the standard deviation, and the percentage of F's falling within the theoretical measure of dispersion — approximate those demanded by the normal probability curve, it is believed that the error is reasonably well within the error of sampling. Therefore, it may be concluded that the data obtained by intercorrelating the achievement in the different departments tend to support the concept of general ability as proposed by Spearman.

[11] C. Spearman and Karl Holzinger, Note on the samplimg error of tetrad differences, *Brit. J. Psychol.*, 16, 1925, 86-88.

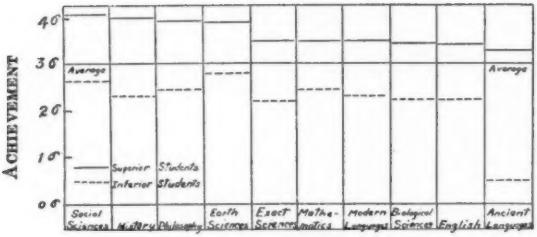

FIG. 1. RELATIVE ACHIEVEMENT OF EXCEPTIONAL EXACT SCIENCE STUDENTS

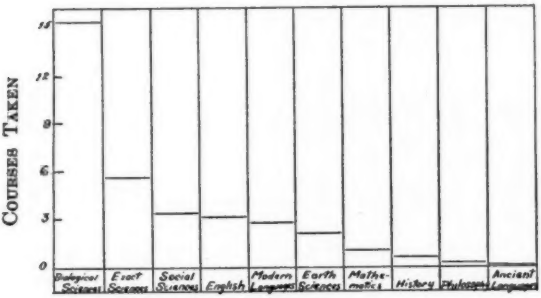

FIG. 2. AVERAGE NUMBER OF COURSES TAKEN BY BIOLOGICAL SCIENCE STUDENTS

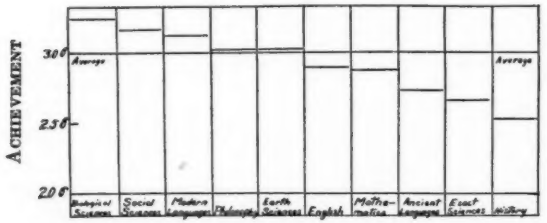

FIG. 3. RELATIVE ACHIEVEMENT OF BIOLOGICAL SCIENCE STUDENTS

(2) For the second method an analysis was made of the achievement of exceptional students. In a given departmental group the students were ranked with respect to achievement in their major department. Then all students in the upper fifth and all those in the lower fifth were selected. These two groups were compared with respect to their achievement in each department. Fig. 1 shows the relative achievement of exceptional exact science students. It shows that the student who is poor in scholarship in his own department is inferior in every other department and likewise the student who is superior in his own department is superior in every other department.

The same procedure was used for each of the other departmental groups and this tendency was found to be true for each group. The results agree with those obtained by Blackhurst,[12] who found in a study of school grading that, after leaving out such subjects as music and physical education, the bright pupil in high school or college will stand high in all subjects, the dull low in all subjects, and the intermediate around a corresponding point in all subjects.

It may be concluded that in so far as academic courses in college call forth the best efforts of the student and to the extent that teachers' marks are discriminative as to ability, there exists a general ability or disability on the part of students for meeting the intellectual demands of the various types of subject matter. It may be assumed on the basis of this conclusion that differential scholarship in the various departments means differential interests and that scholarship varies roughly in direct relation to interest.

The Data. The data for this study were provided by the records of 758 students in the College of Arts, Literature, and Science, of the University of Chicago. Only students who had completed the baccalaureate degree and only those who had done all of the four years' work in residence were included.

The various departments of instruction were grouped into fairly homogeneous groupings for convenience as follows: mathematics, exact sciences, biological sciences, earth sciences, ancient languages, modern languages, English, history, social sciences, and philosophy.

In order to secure comparable measures of achievement in the various departments all teachers' marks were transmuted to 'standard scores,' that is, each mark was expressed in terms of the standard deviation of the distribution of marks in the department in which it was given.

Results

The results of the study are somewhat conflicting. There is a moderate degree of agreement between the two criteria of interest, as is shown in Figs. 2 and 3, which are representative of tendencies in selection of courses and scholarship gained. The average coefficient of correlation between achievement in the different departments and the courses taken is 0.34. It is impossible to give as clearcut and concise a characterization of any departmental group as was desired. On the other hand, there are revealed many interesting and illuminating facts, which it is hoped will be of some interest to the reader. They will now be considered, first taking up those concerned with selection of courses and lastly those dealing with relative achievement.

(1) *Selection of Courses.* Table 1 shows the extent to which students choose the various intellectual fields for intensive study. This table shows the strong tendency of students to select one of the social sciences as a major subject notwithstanding the fact that this study does not include those students who were enrolled in the School of Commerce and Administration and in the School of Social Service Administration.

[12] J. H. Blackhurst, The normal curve as applied to high school and college grading, *School & Soc.*, 13, 1921, 447-450.

TABLE I

Distribution of students with respect to the departments in which they take their principal sequence

Department	Frequency	Percentage
Social Sciences	176	23.21
English	154	20.32
History	103	13.59
Exact Sciences	85	11.21
Modern Languages	70	9.23
Earth Sciences	66	8.71
Biological Sciences	36	4.74
Mathematics	33	4.35
Ancient Languages	27	3.56
Philosophy	8	1.06
Total	758	99.99

If all the students are divided roughly into 'humanitarians' on the one hand and 'scientists' on the other, it is found that the scientists seek a wider range of experiences than do the humanitarians. These results are represented graphically in Fig. 4. Whether conditions shown here represent the most desirable balance between 'constants' and 'electives' is problematical. Administrators for many years have been concerned with the question as to the extent to which students should 'concentrate' and 'distribute' their efforts during the college course. But, if the writer were to venture a suggestion it would be to the effect that the humanitarians should secure more training in the sciences. If this could be done without changing the requirements but rather by increasing the drawing power of the sciences, then so much the better. As Rigg has pointed out, the sciences certainly do not lack in cultural value.[13]

With the exception of the ancient language group, few of the students select to any extent courses in the ancient languages. The maximum average number of courses selected by any departmental group, excluding the classics students, is less than one-half for each student. All students tend to take more courses in English and the social sciences than are required for graduation.

Fig. 2 is a graphical representation of the way in which biological science students select their courses and is quite typical of the students in general. All curves showing the distribution of courses taken in the various departments by the different departmental groups are high and steep in the vicinity of the major and closely related departments. There is a strong tendency toward specialization despite the fact that the students were doing their work in an institution which allowed a great deal of freedom in the selection of courses.

(2) *Relative Achievement.* When the various departments are arranged in a hierarchy with respect to scholarship gained in each by a given departmental group it is found that the department in which the group are taking the major sequence always stands at the top or near the top, for example, see the biology group in Fig. 2. One condition which prevents the order of courses from being what would be expected in connection with any departmental group is the almost universally high position held by the social science department. This is accounted for largely by the fact that scholarship of the social science students is quite low as compared with that of other groups. The achievement of these students is below the average in every department including their own. This group

[13]G. B. Rigg, Science and liberal education, *School & Soc.*, 17, 1923, 477-485.

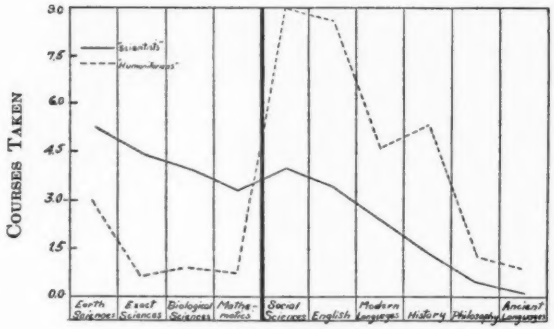

FIG. 4. COMPARISON BETWEEN 'SCIENTISTS' AND 'HUMANITARIANS' IN COURSES TAKEN

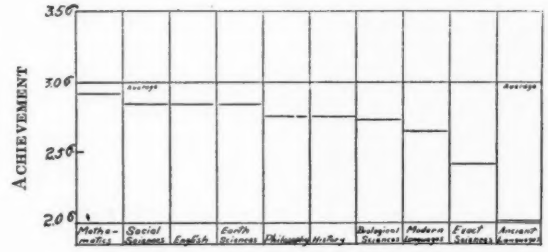

FIG. 5. RELATIVE ACHIEVEMENT OF SOCIAL SCIENCE STUDENTS

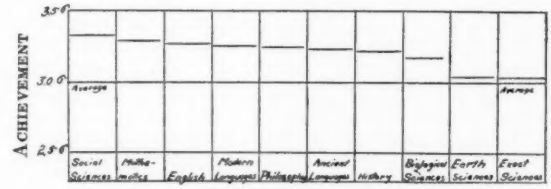

FIG. 6. RELATIVE ACHIEVEMENT OF ANCIENT LANGUAGE STUDENTS

is quite large, therefore, the marks received by other students in social sciences are relatively higher than they would be if the social science students had given stronger competition in securing higher marks in their own department. This low level of scholarship is brought out in Fig. 5.

On the basis of achievement criterion the greatest interest of mathematics students lies in the field of mathematics and its closely related field exact sciences. But apparently there is much less appeal to these students in the realms of history, biological sciences, modern languages, and philosophy. The exact science students gain highest scholarship in exact sciences, philosophy, and social sciences. Their lowest achievement is in ancient languages and earth sciences. The biological science students do their best work in the biological sciences and modern languages while their lowest grades are made in ancient languages, exact sciences and history, as shown in Fig. 3.

Regarding the earth science students there are two interesting facts. First, their absolute level of achievement is very low. It is slightly above the average in their own department but considerably below the average in every other department. Secondly, their achievement in most departments is far below that in earth sciences. It may be concluded from these results that earth science students are inferior in scholarship to the average arts and science student and that their interests are centered largely within their own field.

The ancient language students are the antitheses of the earth science students. They are approximately equal to the average student in two departments and excel the average in every other department, as is shown in Fig. 6. They apparently do not find that the exact sciences and the earth sciences challenge them to do their best, but they gain as high scholarship in these courses as does the average student who takes courses in these departments. Apparently these students are superior students and their interests are quite broad, as indicated by the fact that their achievement is almost equally high in every department.

Much has been said for and against the disciplinary value of the classics. The question is not yet settled, but we are at least justified in assuming that the classics afford a very effective selective agency. Only students with strong intellect would likely survive both the high school and the college course. Davis analyzed the courses taken by 244 Phi Beta Kappa students and concluded that the results did not show that "the greater attention to the study of the classical languages is the sole, or even the chiefest factor in producing superior students." These students were "brilliant and honor students not because they studied more Latin; they may, however, have studied more Latin because they were brilliant."[14] Bolton states in effect that men with strong minds take to the study of languages;[15] they find them interesting. The same type of mind will take to any other studies and success will be achieved in these quite as well. One may conclude from these results that ancient language students are quite superior in scholarship to the average student, and that their intellectual interests extend into practically every division of the arts and science curriculum.

Modern language students make their best grades in modern languages, mathematics, and the social sciences. There seems to be considerably less appeal for these students in all other subjects, although their achievement in general is quite high. They rank second only to the ancient language students in general average in scholarship. English students gain highest scholarship in English, philosophy, and the social sciences. In all other departments their achievement is significantly below

[14]Davis, *op. cit.*

[15]T. L. Bolton, What is the disciplinary value of the classics? *School & Soc.*, 14, 1921, 205-208.

that of the average student. One is struck by their low scholarship in the other languages, their achievement in the ancient languages being at the bottom of the list. History students gain highest scholarship in history, philosophy, and the social sciences, but their grades are significantly low in all other departments.

The data for the social science group present some interesting facts, as shown in Fig. 5. In the first place their achievement is below the average in every department including their own. Secondly, their achievement is rather uniform in all departments with the exception of the foreign languages and exact sciences, where it drops to a very low level. From these results it may be concluded that the social science students are inferior in scholarship to the average student, and that their interests are quite diversified.[16]

The philosophy group was too small to be included in this study, there being only eight students taking a principal sequence in this department.

Summary and Conclusions

(1) Students tend highly to specialize, apparently toward a profession, in an institution in which is given considerable freedom in the selection of courses.

(2) The evidence is quite clear that any given student may gratify the same general intellectual desire equally well in several different departments of instruction. Hence, it is impossible to define, except in a general way, any types of students.

(3) Students who are pursuing a major sequence in one of the sciences tend to seek a wider range of experiences than do those who are interested in the humanities.

(4) There is moderately high positive correlation (0.34) between the number of courses taken and the scholarship gained in a given department, indicating that the student does his best work generally in those courses which he chooses more or less freely.

(5) On the basis of scholarship the foreign language students, especially the ancient language students, are not only superior to all other students but exhibit a wider range of interests.

(6) The social science students show a diversity of interests almost equal to that of the classics students but their scholarship in all departments including their own is considerably below the average.

(7) The earth science students are inferior in scholarship in all departments except the earth sciences and their interests are apparently concentrated largely in their own department.

[16]By way of information it may be stated that the rank order of departmental groups from high to low in general average of scholarship is: Ancient languages, modern languages, biological sciences, mathematics, exact sciences, history, English, social sciences and earth sciences.

THE EXPERIENCE OF HEAT

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The experiment here reported is a continuation of the preliminary study of heat made in the Cornell laboratory by Cutolo.[1] At the time of Cutolo's study two views regarding the experience of heat were current: (1) the view of Alrutz[2] that heat is a unique and further unanalyzable quality conditioned upon the simultaneous stimulation of warm and cold spots; and (2) the view of Thunberg[3] and Kiesow[4] that it is a fusion of cutaneous qualities, particular importance being ascribed by Kiesow to the quality of pain.

Cutolo's stimuli were punctiform and areal. In the punctiform procedure he stimulated adjacent warm and cold spots; the warm by temperatures of 44°-45°C, and the cold by temperatures of 8°-14°C. In the areal procedure he had the ventral surface of the forearm placed on a temperature grill, constructed of 8 glass capillary tubes; he ran warm water, 43°-45°C, through the even-numbered tubes, and cold water, 5°-9°C, through the odd-numbered tubes. His results in both procedures confirmed Alrutz's findings. Heat, he found, is conditioned physiologically upon the stimulation of the warm and cold spots, and is introspectively a psychological unique. It is a simple quality that lies in the qualitative continuum between pressure and pain.

Cutolo's warm stimuli were close to the critical temperatures for pain and paradoxical cold (43°-45°C); he is therefore extremely cautious in his conclusions. "We do not," he says at the close of his paper, "regard the matter as settled. Kiesow's contention that heat can be obtained only with stimuli near, at, or above the pain limen, can be fully met only by reproducing the experiences reported in this paper with stimuli that are beyond the shadow of a doubt inadequate to pain; *i.e.* with a warm stimulus that is inadequate to paradoxical cold, and with a cold stimulus that proves experimentally to be inadequate to ache in the region of the cutaneous surface chosen for experimentation." The next logical step is thus indicated.[5] The temperatures of the cold and warm stimuli must be systematically varied.

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[1] F. Cutolo, Jr., A preliminary study of the psychology of heat, this Journal, 29, 1918, 442-448.

[2] S. Alrutz, Om förnimmelsen 'hett,' *Upsala Läkaref. förh.*, 2, 1897, 340-359; The sensation 'hot,' *Mind*, 7, 1898, 141-144; Studien auf dem Gebiete der Temperatursinne, II. Die Hitzeempfindung, *Skan. Arch. f. Physiol.*, 10, 1900, 340-352.

[3] Torsten Thunberg, Förnimmelserna vid till samma ställe lokaliserad, samtidigt pagaende köld- och värmeretning, *Upsala Läkaref. förh.*, 1, 1896, 489 ff. Cf. also W. A. Nagel's *Handbuch der Physiologie des Menschen*, 3, 1905, 707.

[4] F. Kiesow, Zur Analyse der Temperaturempfindugen, *Zsch. f. Psychol.*, 26, 1901 231-246.

[5] Since Cutolo stimulated only adjacent warm and cold spots, another problem remained: the degree of adjacency necessary for the arousal of heat. J. H. Alston (The spatial condition of the fusion of warmth and cold in heat, this Journal, 31, 1920, 303-312) found that under adequate stimulation relatively widely separated spots (10-15 cm.) still give the simple quality heat. His *Os*' descriptions agree with those reported by Cutolo, and further corroborate the finding of Alrutz that heat is a unique and ananlyzable experience.

Apparatus and Procedure

Since Cutolo obtained identical results in his two procedures—the punctiform and the areal—we chose the areal as being simpler and more suited to our requirements.

Apparatus. We constructed a temperature grill of small copper tubing (1/16 in. bore), making the cold (A) and warm (B) systems separately. Each system consisted of a piece of tubing bent, without kinks or buckles, into 10 parallel sections every one 14 cm. in length; the water flowed freely and unobstructedly through each system. The turns of system A were bend down so that the two systems, when interplaced and fastened to the asbestos-covered cleats on the arm rest, lay in the same plane. At the cleats the systems were isolated from each other by asbestos, and between the cleats, which were 8 mm. high, by air spaces of approximately 1 mm. The stimulating surface was the area of the grill between the cleats, 8×8 cm.

The water, forced through the systems by the pressure of the University mains, was led to the grill systems through copper coils, kept in constant temperature cold and warm water baths. We regulated the flow of water from the mains, and, by means of electric heaters and fans, we maintained the temperature of the room at 25°C ± 0.5°. As a result the temperature of the grill systems remained constant for any given setting to within 1/10°C. During the preliminary set-up we took the temperature of the water both before it entered the grill systems and after it had passed through. After the differential between the temperatures at the intake and exit had been established, we took the temperature of the water only at the exit. We adopted this procedure so the thermometers at the intakes—at the forward end of the apparatus—could be dispensed with as they would have interferred with the *O*s' freedom of action. The thermometers at the exits were suspended so that the streams of water, squirting from the grill systems, bathed the mercury bulbs.

Procedure. We had 4 temperatures for each system: 2°, 10°, 20°, and 30° for the cold system A; and 34°, 37°, 40°, and 43° for the warm system B. We used every one of the cold temperatures 5 times with every one of the warm temperatures.

When *E* had brought the systems to the proper temperatures, *O*, seated at a table and to the left of the grill, placed his left hand on a rest; at a signal he brought down the volar surface of his left forearm and rested it, with the pressure of the arm's weight, upon the grill. After 15 sec. he removed his arm and wrote his report. Meanwhile, *E* adjusted the temperatures for the next experiment.

Observers. Our *O*s were Dr. Elizabeth F. Möller (M), honorary fellow in psychology, Dr. Harry Helson (H), instructor in psychology, and the senior author (D). H and M worked without knowledge of the problem. All the *O*s were acquainted with Titchener's touch pyramid.[6] How far they may have been influenced by this classification we do not know with certainty; their reports, however, show that they were affected little if at all, and that they approached the observations without bias.

Instructions. At the beginning of every experimental hour *E* read the following instructions to the *O*s: "At the signal 'now' place the volar surface of your left forearm upon the apparatus. Keep your arm steady. Remove it at the second signal 'now,' and write your report. Describe the course and the quality of your cutaneous experience during the experimental interval."

[6] E. B. Titchener, Models for the demonstration of sensory qualities, this Journal 31, 1920, 213.

Results

The number of times that every *O* reported heat at the various temperature combinations is shown in Table I under the cap-

TABLE I

Showing the number of reports of intense heat and mild heat and the total number for every *O* at the various stimulus-temperatures.

Stimulus Temperature		OBSERVERS								
		D			H			M		
A	B	Intense	Mild	Total	Intense	Mild	Total	Intense	Mild	Total
30°	34°	0	0	0	0	0	0	0	0	0
	37°	0	0	0	0	2	2	0	0	0
	40°	0	1	1	0	5	5	0	1	1
	43°	0	4	4	5	0	5	0	2	2
20°	34°	0	0	0	0	4	4	0	0	0
	37°	0	2	2	0	4	4	0	0	0
	40°	0	4	4	0	5	5	0	0	0
	43°	4	1	5	5	0	5	3	0	3
10°	34°	0	0	0	2	3	5	0	1	1
	37°	4	1	5	3	2	5	0	3	3
	40°	3	2	5	3	2	5	3	1	4
	43°	5	0	5	5	0	5	4	0	4
2°	34°	1	3	4	5	0	5	0	2	2
	37°	2	3	5	4	1	5	2	1	3
	40°	3	2	5	5	0	5	2	2	4
	43°	5	0	5	5	0	5	5	0	5
Total		27	23	50	42	28	70	19	13	32
43°	43°	0	0	0	2	3	5	0	0	0

tions 'Total.' All the *O*s report heat at warm temperatures far below those adequate to pain and paradoxical cold, and at cold temperatures far above those adequate to ache. For example, they all report heat at 34° given in combination with 2°, and at 30° with 40° and 43°. At the intermediate temperature-combinations, in which neither of the extremes of warm and cold appear, all again report heat. When for example, system A is at 10°, H reports heat at all 4 temperatures of system B; D and M at 37°, 40°, and 43°. Again, when system A is at 20°, H reports heat at all the temperatures of system B; D at 37°, 40°, and 43°; and M at 43°.

Reports of heat at temperatures that are totally inadequate to ache, pain, and paradoxical cold clearly indicate that these qualities are not the *sine qua non* of the experience. Kiesow's contentions are fully met; Alrutz's findings are again confirmed.

Quality. The introspections, again, bear out the view that heat is a unique quality. We give below excerpts from the *O*s' reports at the various temperature combinations.

D, (temperatures 30°-43°) "Cool, of momentary duration; then sudden and abrupt change to warm heat, that is to a warm that is slightly pricky and puckery or drawy. This quality is there in addition to the warm. It soon disappeared, leaving the smooth comfortable warm behind, and this in turn gradually faded in intensity."

(20°-43°) "Cool momentarily, and then hot. This rapidly grew in intensity, and then toward the end of the experiment declined. During this experiment I gave my entire attention to the quality of heat. The words that I have hitherto used to describe it do not fully convey the character of the experience. 'Stingy' is too gross; hot does not have a gross character. It seems intimate, 'Drawy' is too mild. The quality isn't coarse, but neither is it lamb-like. It is pressury without a doubt. It is an astringent smooth pressure, to use a new combination of adjectives."

(10°-43°) "Cold of short duration, then a short stage of indifferent pressure; then intense heat, pricky and drawy in quality, which persisted throughout the rest of the period."

(10°-37°) "Intense cold, followed by a blush of warmth, and then by a puckery, drawy heat. It was intense, and seemed ever on the point of burning. A stingy, pressury quality."

(2°-37°) "Cool for an instance, then a warm heat. This change was abrupt. Then the warm heat became less and less warm, until the heat stood alone. This change was gradual. The heat became more and more intense, until at the end of the experiment it was very intense. The quality of the heat was at first pricky, drawy, tingling. These qualities became more and more intense as the intensity of the heat advanced. At the end of the experiment a new quality of smart was added to the heat."

(2°-34°) "At first cold; this very soon disappeared. It was followed by a period of indifferent pressure, and then by a blush of warm which lasted but a brief interval. This was followed by a mild heat which was of a pricky, drawy, tingling quality. Cold and warmth were both absent; only this heat was present."

H, (temperatures 30°-40°) "A mild hot: tingle, penetrating components and bright pressures constitute the body of the experience."

(20°-37°) "Immediate experience of heat. Tingling, bright prick and sting components—all very surfacy."

(20°-34°) "First cold, then hot. The hot took a moment to come to its full intensity, and then was a typical hot—a pricky, tingling, surfacy quality."

(10°-43°) "A clear unmistakable hot—surfacy, bright, and penetrating, with sting and prick components."

(10°-34°) "A suffused, intense, penetrating heat—pricky, burny, and stingy."

(2°-43°) "Intense heat—burn, prick, sting and bright pressures. The experience is a piercing one, penetrating from without in—a sort of blanket of heat which bright pressures, and pricks, and stings obtrude themselves."

(2°-34°) "Intense heat with smart, tingle, prick, and bright pressure components. Penetrates and bites in; the more intense the heat, the deeper the bite."

(2°-37°) "Intense, stinging, biting heat with prick, tickle and bright pressure components. The first shock was the worst, eating into the arm and then subsiding into a constant sting and heat. The experience maintained a constant quality after the first intense experience. This intense heat was penetrating, from without in, with bright quivery components strongly in evidence. The bright pressures almost verge on pricks, being like little needles pressing in a little way. The experience seems to be punctuate rather than smooth and continuous."

(2°-37°) "First a 'welling-in' of heat which increased in intensity, and then a slight decrease in intensity, but still preserving the heat quality. Bright pressures, sting, prick, and pain constituted the body of the experience; it remained very steady after the first rise and subsidence."

M, (temperatures 30°-43°) "First a cool neutral pressure which is indefinitely bounded. The cool is of a very low degree of intensity—barely cooler than body temperature. Almost immediately another experience appeared—a pricky pressure, not particularly intense, but with a suggestion of pain in it, so that it appeared at first as heat. The prick and pain components were apparent later."

(20°-43°) "Cold neutral pressure, then heavy prickly pressure with a little cloud of warmth around the edges. The pricky pressure was faintly unpleasant, faintly painful, and was perceived as heat. Of the qualities of cold, warm, heat, and pressure I can speak with certainty. They were clear as qualities. The heat experience, however, was dominating—its intensity was so great that it tended almost completely to overshadow the other experiences."

(10°-40°) "A neutral pressure which at first appeared to be warm—a welling, active warm—with an upper boundary of cold. A part of the warm experience changed to heat, *i.e.* to a pricky, painy experience that wasn't a real prick or a real pain, and that was faintly unpleasant. But around the heat there was a kind of aura of smoky, moving, welling warm which was pleasant."

(2°-43°) "At first an indefinite but limited area of neutral pressure which is also cold. The cold is rather intense. After a short time an experience of warm seems to well up inside the cold. The warm increases rapidly in intensity, and suddenly is gone, except for a faint suggestion which forms an outline around an intense painy, pricky pressure which occupied the position of the previous warm. This pressure, during the experience, moves closer to pain and farther away from prick. It is the experience known as heat, though I take it as a pricky-pain when it occurs. The cold, which surrounds the warm outline of the heat, increases in intensity, until it too seems more pressure than cold. It lies flat, but something sharp and light throbs in it. This is almost like the heat experience. It is just enough different to be different. The heat-pressure is lighter and more active and twinkling; the cold-pressure is slow and rhythmical in its recurrence, and it is heavier."

(2°-40°) "There was an indefinite neutral pressure to which was immediately added an experience of cold. Then warm arises gradually, welling up. After a short time the warm disappeared, except for a small bit of it. In its place there appeared a pricky, painy pressure that was not really prick or pain, though it was like both; it was, in other words, heat."

In the series of 80 experiments, D reports heat, as shown in Table I, 50 times. His characterizations of this quality are: drawy (33), puckery (25), pricky (17), stingy (11), pressury (10), astringent (6), tingling (5), smooth (2), burning (2), smarting (2), soft (1). (The numbers in parentheses indicate frequency of use.) 'Drawy,' 'puckery,' or 'astringent' appear in 44 of the 50 descriptions of heat. 'Stingy,' 'burning,' and 'smarting' refer to intense experiences only; 'pressury,' 'tingling,' 'smooth,' and 'soft' only to mild. For D, therefore heat is an astringent quality that lies in the qualitative continuum between pressure and pain, close to prick and nearer to pressure than to pain.

H reports heat in 70 of the 80 experiments. His characterizations are: prick (53), sting (42), bright pressure (30), tingle (25), burning (19), pain (16), biting (10), smarting (7), piercing (4), pressury (4), tickle (2), itchy (2), ache (2). The first four terms are most characteristic of his descriptions; the others are, for the most part, attempts at differentiating the intensive aspects of the experience. Heat, for H, seems therefore to be a tingling, pricky, stingy, bright pressure quality that lies in the pressure-pain continuum, closer to pressure than to pain.

M characterizes the heat experience, which she reports in 32 of the 80 experiments, as prick (29), pressure (28), pain (21), light (4), lively (4), active (3), heavy (3), sharp (2), and throbbing (2). Except for the early reports she characterized every experience of heat as a 'pricky, painy pressure;' the other terms describe the particular shadings of the pricks, pressures, and pains that were now and then observed. Heat very definitely lies for M in the pressure-pain continuum, near prick and closer to pressure than to pain.

Absolute agreement upon the characterizations of heat is not to be expected. If heat is a unique and simple quality, its description is an impossibility. All that one can do when called upon for a description of an ultimate is to exhibit it, or to name qualities that closely resemble it. Consider the situation facing a naïve *O* who is called upon to describe, say, a reddish orange—a naïve *O* because any *O* is necessarily naïve over against the world of touch as long as the qualitative series are not definitely established. Faced with the task of describing reddish orange this *O* might list the qualities that resemble it—and his competency would be shown by his ability to do this—but he would find it an impossible task to describe it. Similarly in the case of heat. Our *O*s find heat to be a simple experience, and their characterizations are in close agreement; they thus confirm the findings of Alrutz,[7] Cutolo,[8] and Alston[9] that heat is a simple quality that lies in the pressure-prick-pain continuum, near prick and closer to pressure than to pain.[10]

Intensity. Though the *O*s were not instructed to report upon intensity, they nevertheless did so. We were able to classify their reports into intense and mild; the number of each class for every *O* and for every stimulus-combination is shown in Table I. It will be noted that the number of reports of heat and the proportion of reports of mild and intense heats vary

[7]Alrutz, *opp. citt.* [8]Cutolo, *op. cit.*, 455 ff. [9]Alston, *op. cit.*, 311.

[10]These results stand in disagreement with Titchener's touch pyramid (this JOURNAL, 31, 1920, 213), in which heat is placed as an intermediate between neutral pressure and prick, and pain as an intermediate between prick and ache. The location of heat is approximately correct—it lies closer to prick, however, than Titchener has represented it—but the location of pain is wrong. Pain is not an intermediate, but a terminal quality. These experimental findings necessitate the recasting of the figure.

with the differential of the stimulus-combination. Reports of heat are few when system A is at 30° and the differential small, but there are many of them when the temperature is 2° and the differential large. The absolute as well as the relative number of reports of mild and intense heat varies similarly with the differential.

Although our procedure does not permit us to make a direct comparison of the intensities of the various experiences of heat, and thus to determine which temperature combination conditions the most intense heat, the distribution of the *O*s' reports indicates that the larger the differential the greater the heat. When system A is at 30°, D and M report no intense heats and H—whose case we shall consider later—only a few; all the *O*s report the greatest number when system A is at 2°. Over and above this statistical evidence, all the *O*s describe the experiences of heat conditioned by the stimulus-combination 2°-43° as the most intense of all. When this combination was first given, D reported, "This is the most intense heat that I have as yet experienced;" H reported, "Experience of great intensity;" and M reported, "A very intense painy pricky pressure."

The intensive increases are characterized differently by the 3 *O*s. D says, "The quality of heat at the times of greatest intensity becomes more stingy, " "seems ever on the point of burning," "is smarting;" H describes the intense experiences as "burning," "biting," "smarting," "piercing," "painful," and "achy;" and M simply reports an "intense" or "very intense" painy pricky pressure. The intense experiences seem to differ in kind for D and H, and merely in degree for M. D's, and in a greater measure H's, intense heats are marked by an increase in the pain component. It is as if the quality of the intense experiences had shifted toward pain in the pressure-pain continuum.

The intense heats that H characterized were reported at the higher temperatures of system B. At 43° H invariably reported an intense heat. It is possible therefore that this temperature was not below H's critical temperature for pain and paradoxical cold, and that the qualitative shift noted by H in the intense experiences indicates the arousal of pain and the fusion of that quality with the true quality of heat.

In order to test this explanation we carried out a supplementary series of 5 experiments with both systems at 43°. The results are shown in Table I. D and M do not report heat; their experiences fall within the intensive continuum of warm.

D, (43°-43°) "At first blush it was an intense warm, then rapidly declined to a moderate warm, and then to a weak warm. The warm was confortable, snug, smooth, and soft."

M, (43°-43°) "The warm at first was rather intense, then it grew less and less so, but never entirely disappeared so that at the end of the experience I had a warm that was not as intense as it had been at the beginning."

H, however, reported heat in every experiment; and his descriptions in no way differed from those given in the regular series, as the following example shows.

H, (43°-43°) "Hot stingy burn with a background or matrix of warmth. The burn is penetrating and biting with bright components. It grew less intense as the experience continued. The matrix of warmth refers to the fact that behind the bright, stingy, pressury quality there was a warmth, glowing and solid in nature."

The temperature of 43° is thus found to be adequate for H to paradoxical cold and to pain. It is therefore probable that with temperatures of 43° in the regular series pain accrued to heat, and was reported as an increase of the pain component.

The explanation of the qualitative shift in D's reports on intense heat is not so clear. It should be noted, however, that the shift was not as marked in his reports as in H's. For D, the intense heats did not burn, they merely 'seemed on the point of burning;' they did not become more painful, but merely 'more stingy,' and 'smarting.' Heat is not a point upon the qualitative continuum any more than is orange. We have reddish oranges and yellowish oranges and so also we may have more or less pressury and painy heats. D's report may merely mark the qualitative dispersion of the heats in the pressure-pain continuum. We have no other explanation.

The intensity of the heat experiences was discovered by D to be conditioned in part by the pressure of the arm upon the grill. After the 39th experiment, with temperatures of 10° and 42°, he reported:

"Heat throughout the course of the experiment. It fluctuated in intensity. I observed, however, that the fluctuations could be produced by varying the intensity of the pressure of the arm upon the grill. I have been partially supporting the weight of my arm by my shoulder muscles. If I relax my muscles completely and let the entire weight of my arm bear down upon the grill the intensity of the heat is much greater. As the pressure upon the grill is varied so also is the intensity of the heat varied."

We seem to be dealing here with a phenomenon similar to that which occurs when electrodes are grasped loosely or tightly. How far the intensities reported by our *O*s are conditioned by the pressure of the stimulus we cannot say, as this was the only report that bore upon this point. We tried, however, to eliminate this variable by instructing the *O*s to relax when their arms were upon the grill.

Individual differences. We were guided in the selection of our stimulus-temperatures by the work of the other investigators. We selected values for system B which, we thought, would be adequate only to warm. That we did not succeed in doing this was, as it turned out, a fortunate error because it

brought out the fact of individual differences, which now enables us to explain the diversity of results in the earlier investigations. Alrutz's warm temperature was 44°, Thunberg's and Kiesow's 45° plus, Cutolo's 43°-45°, and Alston's 43°-44°. If true heat experiences are to be studied, temperatures adequate to pain, ache, and paradoxical cold must be avoided. Thunberg's and Kiesow's *Os*, 2 of Cutolo's, and one of Alston's reported that the experience of heat was painful. Thunberg's and Kiesow's temperatures were above the critical point for pain; consequently the resulting experience would be a heat-pain fusion. Cutolo's and Alston's temperatures were presumably below the critical point, which is considered to be 45°, but we have no assurance of that. If Alrutz's, Cutolo's, and Alston's *Os*, like M, had a high temperature-pain limen. They would experience heat alone (at their temperatures); but if, like H, they had a low limen, they would experience pain in addition to heat (at their temperatures). In the first instance heat would be characterized as unique and simple; in the second it would be regarded as a fusion or complex in which pain played an important rôle. Individual differences may no longer be neglected in the selection of temperature stimuli for the arousal of heat.

Temporal aspects. All the *Os* report that, apart from pressure, the quality of cold most frequently precedes in experience. This result stands in agreement with those of Cutolo[11] and Alston,[12] and is probably due to the different latent periods of cold and warm. The cold may be followed by warm and then by heat; immediately by heat without the warm interlude; by a period of indifference and then by warm or heat or both; by a neutral pressure and then by heat; or several of these qualities, spatially patterned, may appear simultaneously. The progression is so irregular that we are unable to generalize. The only uniformity that our data suggest is a tendency for heat to be reported early when the stimulus-differential is large.

Spatial characteristics. At times, as we have just said, several qualities were observed together, not as fusions but as distinct experiences which ran their courses independently.[13]

D, (10°-37°) "The pricky drawy experience (heat), was confined to the central part of the stimulated area, the sides were warm, and the peripheral end was cold." (20°-40°) "The parts along the side and back were perceived as warm, the center and front was perceived as hot."

H, (10°-34°) "Hot, stinging burn, with cold edges." (20°-37°) "An experience that was hot and cold—not mixed. The cold in an area near the wrist, the hot in the fleshy part of the arm."

M, (10°-37°) "There were four experiences present at one and the same: a pricky pressure (heat), a cold pressure, a warm experience, and a neutral pressure." (2°-37°) "There is an indefinite area of cold pressure. Beside

[11] *Op. cit.*, 444 f. [12] *Op. cit.*, 305.

[13] Cf. Alston, *op. cit.*, 311.

it there appears a warm neutral pressure. Then experience changes, and I got an outline of warm within which appears two sets of pressures which are much alike but different. One set is prickly, light, active, bright and a bit like pain (heat); and the other set is bright, but heavy and solid (cold).

D and H localized the experiences of heat on the arm.[14] D always referred the heat to the surface. "The experience is surfacy." "The puckery, drawy, astringent, pressury sensations seem to be on the surface." H at times described the experience similarly: "heat is a surfacy, thin, filmy experience;" "it is surfacy and spread out;" "it is like a thin film about 1/4 mm. in thickness;" but at other times he reported that the experience was tridimensional. "The pricks and stings and pain components bite into the arm," "they penetrate," "seem to dig in from the outside," "to dart in from an even layer on the surface," and under these conditions "the experience becomes voluminous and gains a third dimensionality." This penetrating character was reported by H only when the temperature of 43° was used. Since this temperature is, as we have shown, above H's pain limen, we believe that the tridimensionality that he reports is due entirely to the fusion of the qualities of heat and pain, and is not a characteristic of heat itself.

M at times localized her experiences on the forearm—"experiences there on the forearm," "seemed to well-up from within"—but for the most part her experiences do not have a local reference; they are detached from her body,[15] and seem to be pure experiences.

(10°-40°) "I localize these [warm, cold, and heat] qualities in relation to each other in a sense, and also to myself. They are 'out there' in a field which I realize now is restricted, but at the time experienced seems all of existence; and they fit into each other as black smoke appears in a cloud of white smoke. There is a going-on-ness about them, as if they existed forever, yet there is no 'forever' in consciousness. They seem to be pure experiences, which would go on whether I was there to observe them or not."

(10°-37°) "These experiences are localized 'out there' some place in a small area that is the whole experiential world. I realize now how small it is; at the time, however, I don't think anything about it. It is all unreal, unrelated to me, except as it is 'out there.'"

(10°-37°) "The experiences occurred 'out there' where I could look at them. Though they occupied a rather small area—when one considers the world at large—yet that small area seemed all of existence. This is peculiar. It's as if one looked at a patch through a tube held rather close to it."

(10°-43°) "These [warm, cold, and heat] are localized in reference to each other and 'out there.' Ridiculous as it sounds, they do not occur in the wrist until I start to describe them, and then only casually. The things I am talking about are 'out there,' but I don't know where. They have limits, I know, but I can't find these limits, or anything beyond them. I don't, however, think of all this while I'm having the experiences."

[14] So also were they localized by Alston's *Os*, B and P, *op. cit.*, 210.

[15] As Alston's observer Y, *op. cit.*, 310.

After-Images. The after-experiences that followed the removal of the arm from the grill were so pronounced and so insistent that the *O*s frequently mentioned them, though not called upon in the instructions to do so. D and M mentioned them casually, describing them in only 14 and 17 reports respectively; but H was more consistent, mentioning them in 78 of the 80 reports.

D reported after-images of heat twice, of burning heat 3 times, and after-experiences of wetness 9 times.

(2°-37°) "After-images of heat are intense and of long duration. Even now as I write the pricky, drawy, qualities of heat play back and forth over the area of my arm that was stimulated."

(20°-37°) "The quality of burn, a stingy, smarting quality, more pronounced in the after-image than in the perception."

(20°-34°) "When the arm was removed I thought it was wet and actually examined it as well as the arm-rest to see whether there was any water upon them."

(10°-34°) "When arm was removed I had an illusion of wetness. The illusion so strong that I had to examine my arm before I could be convinced of the contrary."

(2°-34°) "After-image of wetness. This so strong that I examined my arm to see if perchance a few drops of water were not adhering to the skin."

(2°-34) "After arm removed I had an unusually clear perception of wetness. I examined my arm fully expecting to see it dripping with water."

D reported wetness only at the combinations 2°-34°, 10°-34°, and 20°-34°. A greater degree of warmth than 34° seemed fatal to the illusion which, in line with the work of Bentley,[16] Thunberg,[17] Tung,[18] and Bershansky,[19] appears to be conditioned by the positive after-images of cold and pressure.

H reported after-images of burning heat 53 times, of heat 21 times, of warm twice. Twice, at the temperature-combinations 30°-34° and 30°-37°, he reported the absence of after-images; and in the two remaining reports of the 80, at the same combinations, he made no mention of the after-images. The two after-images of warm were reported at 20°-34° and 30°-37°; the after-images of heat were reported at 30°-34°, 30°-37°, 30°-40°, 30°-43°, and 20°-34°; and of burning heat at all the other combinations.

In H's case conditions of the post-experimental period seemed particularly favorable for the experience of heat. In 4 experiments in which heat had not been experienced during the experimental interval it was reported as an after-image. Examples are:

[16]Madison Bentley, The synthetic experiment, this JOURNAL, 11, 1900, 414-425.
[17]T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen*, iii, 1905, 708.
[18]S. Tung, The integration of punctiform cold and pressure, this JOURNAL, 32, 1921, 421-424.
[19]Ida Bershansky, Thunberg's illusion, this JOURNAL, 34, 1923, 291-295.

(30°-34°) "A flash of cold at first then nothing except pressure... The after-image is a burning, welling experience."

(30°-37°) "Experience of warmth that was almost heat but not quite because the prick and tingle and pain components were not strong enough. The warmth is full, voluminous, smooth and agreeable.. The after-image is one of heat."

H's after-images of heat in some of the experiments "are less intense and pale as compared to the originals;" in others they were of approximately equal intensity, "just like the originals;" while in still others "the after-images are hotter," "more intense," "burn, tingle, and hurt more than the originals," *i.e.* the heat aroused during the experimental interval.

The after-images of heat "waver," "come, and go" "fluctuate," they are "steady," "constant;" they are "of short duration," "of long duration," "persistent;"[20] and they are "surfacy," and also "penetrating." Our data are too few to determine the cause of this variability; we merely note the divergence.

M reported after-images of heat 7 times, cold 4 times, cold and prick twice, cold and warm twice, and of warm alone twice. These experiences were reported only incidentally.

(10°-34°) "There is even now a positive after-image of heat."

(2°-40°) "There is a positive after-image of cold."

(2°-43°) "There is a positive after-image of the cold and the prick."

(2°-40°) "I still have a positive after-image of cold and warm."

(30°-43°) "There is a faint after-experience of warm—but it is almost gone now."

The after-images of heat that M reported followed experiences of heat, but not every experience of heat was followed by after-images of heat. Too few after-experiences were reported to enable us to draw a correlation between the nature of the experience and the resulting after-image. We must be satisfied here merely with recording the various after-experiences that M reported.

Synaesthesis. M gives descriptions of her warm and cold experiences that are synaesthetic in type. "It is funny," she writes in one of her early reports, "but these things (warms and colds) suggest pressure and gray. I want to describe them, not in terms of warm and cold, but as curious pressures that aren't related to me, but are 'out there' in front of me and are gray." She consistently throughout the experiment described the warms as "smoky," "dark," "cloudy," and the colds as "misty," "grayish," "foggy."

[20]The after-images of heat were frequently of extremely long duration. H once reported that "the burn, tingle, and prick persisted all afternoon and into the evening. At 4 o'clock I looked at my arm to see if a blister was there, so real and clear were the after-images from it."

(10°-37°) "The cold is grayish, misty, like a thin sheet of fog that is almost rain. Then a part of this cloud (the upper left corner) grew bulky, rolling, deep, and rather illusive and darker—this was a soft warm."

(20°-34°) "The cold is thin and white, the warm is soft and gray."

(10°-40°) "The warm welled-up in a dark cloud."

(20°-37°) "The warm became less intense, but it stayed dark."

These expressions are not mere figures of speech used to illustrate the differences between the cutaneous qualities, but are real concomitants of the experiences themselves, as the following report clearly indicates:

(10°-43°) "These experiences, as I get them, are 'out there,' misty and cloudy. They seem as objective as a book on a table. I stand off and look at them. At the same time that they are in a sort of pattern they have no definite boundaries. I can say, 'here is warm,' and, 'here is not warm,' but not, 'here warm stops.' There is a permanence, a stability about them as if they were entirely independent of me."

In spite of this apparent objectivity, M realized—as all synaesthetic subjects do—that the concomitants were imaginal and without objective reality. She gives two reports that bear upon this point.

(10°-37°) "The warm and cold appeared gray—the warm as a very dark blob, the cold as a thin lighter mist. Though they are localized 'out there,' there is nothing gray there, but that's the best way to describe the experience."

(20°-37°) "Two temperature experiences that appeared as cold and warm pressures. Although there is no visual stuff there, the experience can best be described as a large gray square which is rather dense, thin but compact, and in which at the top there is a strip of gray smoke, which at first is very dark, and finally disappears entirely except for a tiny spot."

While M does not positively say that these experiences are synaesthetic, it is obvious that they are:[21] they are stable—warm is always reported as dark, and cold as light; they are localized indefinitely in front—as are the visual concomitants of most synaesthetic subjects; they are imaginal—they are recognized by M to be without objective reality and secondary to the primary cutaneous experiences.

Summary

The results of this study may be summarized as follows:

(1) Stimuli, which singly are adequate only to cold and warm and inadequate to ache, pain, and paradoxical cold, arouse, when combined, the experience of heat.

(2) Physiologically heat is a fusion of excitations that are normal to cold and warm.

[21]The literature upon synaesthesis is voluminous, but it is devoted almost entirely to theoretical discussions of its origin, to questionary studies of its frequency, and to descriptions of the various types. The differentiae of the synaesthetic images have been neglected.

(3) Psychologically heat is a simple quality that lies in the pressure-prick-pain continuum, near prick and closer to pressure than to pain.

(4) The intensity of the heat experiences seems to be dependent upon the stimulus-differential.

(5) The concomitant arousal of pain and heat seems to condition the experience of burn.

(6) The temperature-pain limen varies greatly among different *Os*. Individual differences must be taken into account in work of this nature.

(7) The experience of cold usually precedes the experience of heat, doubtless because of the longer latent period of warm.

(8) Heat may appear in a pattern along with warm and cold.

(9) Heat is variously localized. At times it appears surfacy and on the skin, at other times voluminous and in the arm. One *O* localized it outside of the body.

(10) After-images of cold, warm, heat, burn, and wetness were reported.

(11) Characterizations of the temperature experiences were given by one *O* that are synaesthetic in type.

AESTHETIC PREFERENCE BY COMPARISON WITH STANDARDS

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Following the lines of experimentation with aesthetic preferences described by F. L. Wells in his paper "Reactions to Visual Stimuli in Affective Settings,"[1] this study of choice reactions by comparison with standards was undertaken. A simple chain reaction experiment of the sorting type was devised under the direction of Dr. Wells, in an attempt to observe whether analogous results were obtained when the relatively elaborate technique of the laboratory was not used. The only instrument of measurement used was a stop-watch with which the times were recorded in seconds.

As in previous experiments picture post cards had proved satisfactory stimuli, this type of material was utilized. Selecting the material presented some difficulties, since it was desirable to use such as could readily be purchased in sufficiently large and uniform quantities so that if further work were carried on with the same problem, similar material could easily be found. The group of stimuli consisted of post-cards of buildings and landscapes, and colored papers mounted on cards of post-card size. Important public buildings of the city in which the experiment was conducted were used as one set, regular post-cards ($3\frac{1}{2} \times 5\frac{1}{2}$ in.) being purchased through a news-dealer. Only post-cards on horizontal axes were used. Nineteen cards were given to 10 *Ss*, who arranged them in order of preference, from best to worst. The card falling the greatest number of times in the position of median was chosen as a standard of comparison with 12 other cards, these 12 being chosen from the entire group to include 2 obviously worse, 2 obviously better, and 8 whose differences from the median card were smaller. A set of landscapes, mainly Canadian, comprised the second set, and the same procedure as for the selection of buildings was carried out, except that there was an arbitrary attempt to have a mixed group, including some pastoral and some water scenes.

The saturated colors, red, yellow, green, and blue, were chosen as standards of comparison with the colors. Four-inch squares in sets containing the spectrum colors were purchased from the Milton-Bradley Company. These squares were cut in half, making 2×4 in. rectangles, which were then mounted in the center of white cards $3\frac{1}{2} \times 5\frac{1}{2}$ in. Six colors besides the standard were used, these 6 being varying shades of the saturated color. Among these 6 the attempt was made to have 1 color obviously better than the standard saturated color, 1 obviously worse, and the remaining colors requiring a more difficult choice. The buildings were numbered conspicuously on the back, the landscapes lettered, and the colors marked with a number and letter combination.

The cards were presented to the *S* with standard of each group placed directly in front of him, while the examiner said, "I am going to show you a group of post-cards (or colors) which I want you to divide into two piles, those which you like better than this (indicating the standard), and those which you do not like as well." The reaction-times for each group were taken in seconds. Then those cards indicated by *S* as better than the standard were marked B on a record sheet which had the cards listed according to the order in which they were presented. Similarly those cards designated as worse were accorded a W on the record. The time in seconds for each group was recorded below. A typical record might read as follows:

[1] *J. Exp. Psychol.*, 8, 1925, 64-76.

Buildings		Landscapes	
Card No.	Designated as	Card No.	Designated as
1	B	A	W
2	B	B	B
3	W	C	B
4	W	D	B
5	B	E	W
6	W	F	W
7	W	G	W
8	B	H	W
9	W	I	B
10	B	J	B
11	W	K	B
12	W	L	B
Time 27″		Time 18″	

Green		Yellow	
Card No.	Designated as	Card No.	Designated as
1	W	1b	W
2	W	2b	W
3	B	3b	W
4	W	4b	B
5	B	5b	W
6	W	6b	B
Time 8″		Time 7″	

Blue		Red	
Card No.	Designated as	Card No.	Designated as
1a	W	1c	B
2a	W	2c	W
3a	W	3c	W
4a	B	4c	W
5a	B	5c	W
6a	B	6c	B
Time 7″		Time 6″	

In all, 103 Ss were given the series of buildings, landscapes, and colors. Over half of the tests were given by the writer, and the remainder by an assistant experienced in psychometric methods.[2] The group was a rather highly selected one. It comprised members of the professional class or students, salesmen, and clerical workers or executives, with a few semi-skilled and unskilled workers. The amount of education and training in aesthetic appreciation must necessarily have had an influence on the choice reactions.

The following table, Table I, shows the distribution of ages according to decades:

TABLE I
Distribution of *Ss* by Age and Sex

Age	No. Men	No. Women	Total
Under 20	1	1	2
20-30	23	18	41
30-40	4	13	17
40-50	5	11	16
50-60	8	11	19
60-70	1	7	8
Total	42	61	103

Between the ages of 20 and 30 there were 5 more men than women, but in all other decades women predominated.

Distribution curves of the reaction-times for the total group and for the men and women separately were made, which, however, were largely skewed. Scores were widely scattered, possibly because of too small a group or too selected a group. Then distribution curves were made according to decades, and only in the decade from 20-30, which was also that comprising the largest homogeneous group, was there anything resembling a Gaussian curve. Median times and the mean deviations are shown in Table II.

In general the total reaction-times tend to decrease with each group of stimuli, possibly from increasing familiarity with the task. The sepa-

[2]For help in the development and administration of the test, the writer is indebted to Mrs. Esther C. Whitman.

rate reaction-times for the men and women respectively show a slight exception to this, the men taking a somewhat longer time for landscapes than for buildings, and with no decrease in time between the blue and green colors. The reaction-times of the women were consistently lower with each group except for the last stimulus, shades of red, whose time was somewhat longer than that for the preceding stimulus, yellow. There was, from a superficial point of view, more difference between men and women on the reactions to red than any other color. Subjective interpretation given by certain individuals seemed to point to the fact that the men experienced an immediate sensation of pleasure from the standards of comparison, with a subsequent quick relegation of the remaining shades to the pile designated as worse.

TABLE II
Median Times and Mean Deviations

	Men		Women		Total	M.D. from M.T.	
Cards	M. T.	M. D.	M. T.	M. D.	M. T.	Men	Women
Buildings	33.8	21.8	40	26.5	37	21.9	26.4
Scenes	35.7	14.9	35	22.9	35.6	14.9	23.0
Green	13.7	7.1	14.7	7.5	14.2	7.2	7.5
Blue	13.7	8.0	14.4	8.3	14.1	7.8	8.3
Yellow	13.5	7.4	13.6	8.3	13.5	7.4	8.3
Red	12.2	8.3	14.7	10.4	13.4	8.6	10.4

Because of the wide range of reaction-times, the preponderance of older women, and the greater variations which appeared among the women, the median time by decades of age was worked out as shown in Table III.

TABLE III
Distribution of Median Times and Mean Deviations according to Age Groups

	Age Groups							
Cards	20-30		30-40		40-50		50-60	
	M.T.	M.D.	M.T.	M.D.	M.T.	M.D.	M.T.	M.D.
Buildings	29.3	17.0	27.5	12.7	43.8	13.1	60.0	28.0
Landscapes	29.4	12.3	24.3	12.2	45.0	13.2	58.3	22.7
Green	12.5	6.2	12.5	7.0	17.5	7.1	17.5	9.2
Blue	11.4	5.6	13.3	6.0	14.4	8.3	22.0	11.8
Yellow	11.1	4.2	12.5	8.7	15.0	4.6	20.0	12.0
Red	11.3	6.7	12.8	5.7	15.0	11.2	20.0	13.9

As one can see readily, the times appear to increase fairly consistently with the older *Ss*. However, with the small number of individuals tested and the wide range of times, this cannot be regarded as particularly significant.

The percentage of preference for every individual stimulus was determined, but with little appreciable difference in affectivity between the men and women.

That the men gave shorter reaction-times than the women is contrary to Wells's findings.[3] Possibly the extremely long times recorded of women in the later decades, which tended to raise the median, may account in part for this. Also in the experiment of Wells the number of *Ss* was smaller by far.

Distribution curves were also made on the basis of educational achievement, dividing the *Ss* into three groups—college, high school, and grammar school—and including within each group those *Ss* who had education equivalent to the standard named. There seemed quite a definite differ-

[3] *Op. cit.*, 76.

ence in the reaction-times among the groups. The high school group showed considerably longer times than the college group, and the grammar school group longer times than the high school group. Here again, however, the disproportion of numbers should be considered. Sixty-two of the individuals tested were in the college group, 32 in the high school group, and only 9 in the grammar school group. The median times and median deviations are shown in Table IV.

TABLE IV

Distribution of Median Times and Mean Deviations According to Educational Achievement

	College		High School		Grammar School	
Cards	M.T.	M.D.	M.T.	M.D.	M.T.	M.D.
Buildings	31.3	19.0	45.0	29.6	60	30.7
Landscapes	31.7	16.0	40.0	22.0	75	27.8
Green	13.0	5.4	16.0	7.7	30	15.1
Blue	12.3	7.8	17.1	3.0	25	12.3
Yellow	12.2	5.4	15.0	9.7	25	11.8
Red	12.5	9.1	13.6	8.5	35	11.1

Here again the mean deviations are so great as to make none but the most superficial conclusions justifiable. The difference in times suggested by the table was quite reasonably to be anticipated, for the degree of cultural education has in most cases attuned the individual to a certain degree of aesthetic appreciation and the emotional set is ready for response.

The question arose whether or not the presentation of familiar buildings did not carry some emotional value for different individuals. For this reason pictures of churches had been eliminated. The introspective comments of various persons are interesting as relevant to this point. One young woman who quite obviously was about to choose the picture of a Y. M. C. A. building as worse, suddenly changed her mind, saying, "Oh, that should be counted as better. Think of all the good it does." Again, it was noticed that people invariably designated the State House as better, although the view shown was not particularly attractive, and several times the remark was made that the State House was a beautiful building. It seemed probable that the *S*s were supplementing the picture presented with their own memories of the actual structure. Among the landscapes there was a post-card showing a glimpse of two church spires among the trees, and on looking at this, one individual observed, "I always think churches are pretty." It would seem a better plan, in an experiment of this sort, to present pictures of unfamiliar buildings, and also with the titles eliminated. This would, to a certain extent, limit the opportunity to weight the stimulus with emotional values.

This article has assumed the theory that sensitivity is expressed in shorter time of response. That may be a dangerous premise, and it may be that the more sensitive individual takes longer to give a decision than the one whose aesthetic appreciations are less delicate. Wells observed in the small number of cases used in his experiment that shorter reaction-times were shown by the women, commenting that this result might be connected with the greater affective sensibility recognized to exist among women, whereby the pictures would make a greater affective impression on them. But throughout the study described here, in practically every group the men showed on the average shorter times than the women. This can be regarded as arguing against the theory that higher sensitivity is expressed in shortened times of responses, or as indicating that for the men the pictures presented greater affective differences. Or it may be concluded that neither statement is quite justified, because of the mean deviation, which is so wide as to invalidate any positive inference.

AUDITORY THEORY: A CRITICISM OF PROFESSOR BORING'S HYPOTHESES

By H. BANISTER, Cambridge University, England

In an interesting paper entitled "Auditory theory with special reference to intensity, volume and localization"[1] Professor Boring has endeavoured to "bring the known facts [of auditory theory] into reasonable relation, to point out clearly the lacunae in our knowledge and the discrepances that remain in even the best interpretation, and to formulate the crucial experiments that might resolve these discrepances." He considers that the evidence points to a non-resonance theory of hearing (p.158), and he suggests, further, that a non-resonance theory is easily to be understood if we assume "some kind of integration in the brain where multiplicity of impulses is translated back into degree of excitation" (p.161), and if we accept Köhler's theory of the electrical brain field.

The paper calls for criticism on two grounds, (a) because very doubtful physics is made the basis for wide hypotheses; and (b) because the claim that the resonance theory fails to explain the experimental facts is not fully substantiated.

The non-acceptance of the resonance theory forces the explanation of auditory phenomena back to more central processes. This does not, however, appear to Boring merely another way of confessing complete ignorance. Apparently, if we assume that cortical excitation establishes electrostatic fields, all is clear. I for one am absolutely incapable of accepting such reasoning. It seems to me that with our present knowledge of the functioning of the cortex such explanations are bad physiology and worse psychology.

Boring considers the resonance theory inadequate because (p.158) he does not see how it can explain graduations in intensity: also, in his opinion, the non-resonance theory promises a solution of the 'primary problem' of localisation; the resonance theory, apparently, offers no solution. Hartridge[2] has suggested an explanation of the facts of intensity, while I have shown[3] how the facts of auditory localisation can be explained, on the basis of present-day physiological knowledge, in accordance with a resonance theory.

I do not propose, in this brief communication, to criticise Boring's argument in detail, but shall content myself with pointing out the difficulties in the way of the acceptance of his views. I will begin with those difficulties he himself acknowledges, and then I shall go on to others he has not mentioned.

The first class of difficulties—those which Boring points out—includes (a) Ohm's Law; (b) the length of the refractory period of the auditory nerve fibres; and (c) tonal lacunae.

(a) As regards Ohm's Law, Boring confesses (p.177) that "the hearing by an observer, presented with a complex periodic stimulus, of the tones that correspond approximately to the simple harmonic components of the stimulus as given by Fourier's analysis" is the "great stumbling block to a frequency [*i.e.* non-resonance] theory of hearing." He admits that the law can be explained immediately on a resonance theory.

(b) Boring notes that his theory requires that the refractory period be very low, and he thinks "we may assume that there is a reasonable possi-

[1] This JOURNAL, 37, 1926, 157-188.

[2] H. Hartridge, A vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 12, 1922, 362-382.

[3] H. Banister, *ibid.*, 17, 1926, 142-153.

bility that the refractory period of the human acoustic fibres is as low as 0.05σ" (p.181). This value is about 1/40 the usual estimation. I believe no physiologist would be willing to make the assumption.

(c) Tonal lacunae present a serious obstacle also. Boring surmounts it by suggesting that "recent research, however, indicates that there really are no tonal lacunae in the strict sense. Sensitivity for certain regions of frequency may become diminished, but even here the tones can be heard if they are intense enough" (p.185). It may be a fact that individuals who are normally deaf to certain tones can hear them if the tones are made sufficiently intense, but who is able to say that these intense tones do not affect other fibres of the basilar membrane? It is an experimental fact that even if the fundamental frequency of a complex tone is completely eliminated the tone is heard. It is identified by its over-tones. May not this be the explanation of the fact quoted by Boring? The deduction, that "there really are no tonal lacunae in the strict sense" is scarcely justified.

In this connection some experiments performed by Andréev,[4] in Pavlov's laboratory, are of interest. Five dogs were taken and the left cochlea of each was extirpated. Two of the dogs died almost immediately as a result of the operation. Conditioned reflexes, to tones varying from 100 d.v./sec. to 11,600 d.v./sec., were then established in the remaining three dogs; after which the bone towards the upper end of the right cochlea was opened and the membranous portion of the organ of Corti was injured by means of a thin neddle. On recovery from the second operation the reflexes gradually reappeared. Full details regarding one of these dogs are given. On the fifth day high notes, *e.g.* 8,000 and 14,000 d.v./sec., gave positive results, and gradually responses to lower notes appeared. On the 114th day a tone of 580 d.v./sec. was effective, and 28 days later responses were made to a tone of 360 d.v./sec. Numerous experiments have been made since then,[5] but, so far, 3 years after the second operation, no response has been obtained to a tone below 317 d.v./sec. The second dog is found to be deaf to tones in the middle of the scale. No mention is made in the paper of the third dog. One awaits with interest the results of the histological examination of the cochleae, which will show exactly what portions of the basilar membrane have been damaged.

The second class of difficulties—those not noted by Boring—have to do largely with the facts of localisation. These facts are not so simple as Boring would appear to assume. He points out (p.164) that there are three theories of localisation: the intensity theory, the time theory, and the phase theory. The last theory may be considered as a special case of the second, but the intensity theory, "that localization is towards the side of the ear in which the intensity of the sound is greater" (p.165), is not strictly true, in spite of its popularity among psychologists. If the intensities at the two ears are not equal an observer may judge that a tone of low pitch is localised towards the side at which the intensity is the greater, but, with more careful observation, two sounds will, as a rule, be discriminated, *vide* the experiments of Myers and Wilson,[6] Stewart,[7] and myself.[8] Boring's statement (p.166): "an intensive difference can be equivalent to a time difference and the two can cancel each other in effect," in support of which Stewart's results[9] are quoted, quite overlooks the fact that Stewart found

[4] — Andréev, *Pavlov's Jubilee Volume*, 1925, 339-362.

[5] Personal communication by Dr. Anrep who is in close touch with Pavlov's laboratory.

[6] H. A. Wilson and C. S. Myers, The influence of binaural phase differences on the localisation of sounds, *Brit. J. Psychol.*, 2, 1908, 370 f.

[7] G. W. Stewart, The function of intensity and phase in binaural location of pure tones, *Phys. Rev.*, 15, 1920, 427 f.

[8] H. Banister, Three experiments on the localisation of tones, *Brit. J. Psychol.*, 16, 1926, 266-279.

[9] Stewart, *op. cit.*, 438

that variations of intensity did not always change the localisation. "One image," Stewart remarks on p.428, "did not rotate but remained directly in front, or, by trial, in the position determined by phase difference." Stewart's apparatus was, admittedly, not very exact; but his finding has been fully borne out by other experiments.[10] Since the intensity theory, as expounded by Boring, does not cover these facts, the explanation would seem to fail.

With high tones two and even three apparent sources of sound can be localised when the intensities at the ears are equal.[11] I fail to see how this can be explained by electrostatic fields.

Nor do I understand how Boring will explain the fact that, with sounds of low pitch, time differences at the ears, if they are above a certain maximum, produce ambiguous localisation.[12]

Elsewhere[13] I have enumerated other experimental facts which are satisfactorily explained in accordance with a resonance theory. In order to be convincing, other theories of audition should also be able to explain these facts. The intensity theory, as expounded by Boring, seems incapable of doing this.

On page 175 of his paper Boring states that his theorising "implies that the intensity of a diotic frequency should vary when the phase-relation is varied dichotically," and he goes on to say that an experimental investigation of this relation "would be almost an *experimentum crucis*." In the course of some of my experiments[14] I found that not one of four observers could detect any change in the intensity of a 'diotic frequency' when the 'phase relation' was varied 'dichotically,' though a change of intensity of 10% was easily appreciated. This does not mean that intensity does not change with phase, but the change, if any, could not be appreciated and it was less than 10%. This is, perhaps, as definite a finding as can be expected when the result is negative.

There is one attribute of sounds which I have not so far mentioned, but to which Boring ascribes very great importance, viz., volume. According to him, "volume is at the basis of auditory localization just as visual and tactual extent are basal to visual and tactual localization" (p.176).

Almost everyone who has experimented with sounds has had observers who have reported that sounds possess volume; but it is a very elusive attribute. The volume of any given tone may, my experiments show, vary with the observer; while, with the same observer, the volume may change with pitch, with intensity and with the relative phase at the ears. Reports from Rich's observers[15] indicate that they experienced similar difficulties: they were not at all sure that what they were observing was 'volume.' It is extremely questionable whether the present statues of the attribute 'volume' justifies Boring's statement that "experimental technique brings it [volume] easily under observation" (p.176).

I am not prepared to assert that sounds do not intrinsically possess the attribute of volume, but *a priori* one would not expect sounds to have volume. We do not conceive of light as possessing volume. The object we perceive through the eye has volume; but light has none. Similarly the object we perceive by means of the ear has volume: it is a piano or a person. But one

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[10]S. P. Thompson, *Phil. Mag.*, (V) 4, 1877, 274; Wilson and Myers, *op. cit.*; Banister, *op. cit.*

[11]T. J. Bowkler, On the factors serving to determine the direction of sound, *Phil. Mag.*, (VI) 15, 1908, 318-332.

[12]E. M. von Hornbostel, Beobachtungen über ein- und zweiorhiges Hören, *Psychol. Forsch.*, 4, 1923, 64-114.

[13]Banister, *Brit. J. Psychol.*, 17, 1926, 143 f.

[14]Banister, *Phil. Mag.*, (VII) 2, 1926, 416-431.

[15]G. J. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 151.

would hesitate to postulate that the sound produced when a piano is played, or when a person sings, has volume. A sounding body vibrates, and the energy due to its vibration is transmitted, by means of wave-motion in the air, from the sounding body to the ear. When this energy has aroused nervous impulses in the auditory nerve fibres, and after the various processes necessary to perception have been completed, the sound is heard. However, in no case does the sound exist objectively as a sound. We have sounding bodies, but we do not have sounds as objects of perception. It is customary to say "I hear the sound of a bell," when it would be more correct to say "I hear a bell sounding," *i.e.* vibrating with a certain frequency. It is the 'bell' we hear, not the 'sound.' But we tend to speak of 'sounds' and to treat them as objects, and it seems possible that, as a product of this tendency to reify, we come to consider sounds as being possessed of the usual attributes of objects, and among them of volume.

In addition to this tendency to reify, there is the experimental fact that the direction from which sounds appear to come is often very ambiguous. This ambiguity appears to be usually due to the fact that the sound is heard as coming from two or more directions at the same time. From other data, perhaps, than those furnished by ours ears directly, we know that there is but one source of the sound. Since the sound appears to come from many directions, its seems natural that we should judge that the 'sound' is big, that it occupies some space indicated, perhaps vaguely, by the plurality of directions.

If this is so, 'volume' may be only tantamount to a confession of failure of discrimination. Hence, until further evidence to the contrary is adduced, one might be justified in maintaining that 'volume,' as an attribute of sounds, arises (a) from our tendency to objectify our experiences, combined with (b) the difficulty experienced when we endeavour to localise the source of the sound.

There is another point which seems to be forgotten by many who write on sound localisation. The question is asked: "If we can make judgments of direction with one eye, why can we not tell directions by the aid of one ear alone?" Such thinking is fallacious, for the question too often overlooks the main difference (from our point of view) between these two manifestations of energy, viz., that the wave lengths of light are very minute, while the wave lengths of sound are relatively enormous. In consequence, light waves travel from one point to another, in a homogeneous medium, in a straight line, and it follows that objects are seen only when the eye is oriented so that the light rays fall on the retina. On the other hand, owing to their greater wave length, sound waves may travel from one point to another by innumerable paths. Accordingly, the interposition of a large object between the source of the sound and the ear has, within limits, no effect; the sound is heard whatever the relative positions of the source of the sound and the ear. Obviously, it is impossible for an individual who is blind, and also deaf in one ear, to tell the direction from which a strange sound comes, provided precautions are taken to prevent him from moving his head. If he is allowed to move his head he may be able to detect the direction from which the sound comes by the consequent variations in loudness and timbre. But a normal observer, assuming that the differences between the aerial vibrations at the two ears can be perceived, will obviously be able to discriminate the 'latitude' of a given sound with reference to the line adjoining his ears as axis. He will not, however, be able to make a correct auditory judgment as to the absolute direction of the source of the sound. For this he would require three ears, not in the same straight line. I suggest with all reserve, for my experiments have not yet advanced far enough to enable me to do more than suggest, that this may explain the individual differences found when observers are required to judge the 'direction' of a sound source. Their ears furnish them with information as

to 'latitude:' when pressed to name 'direction' they judge it to be that for which, for some psychological reason, they have a preference. If this is so an inquiry into the reasons for these individual preferences would be of interest.

If our ears can not furnish us with the necessary cues for determining absolute direction, then is it not probable that they are even less fitted for the task of discriminating volume?

In brief, it seems improbable that sounds can have the physical attribute of volume. But, assuming that sounds have volume it appears improbable that our ears can give us any indication of what volume is. Accordingly, it would seem more improbable still that volume can be "at the basis of auditory localization" (p.176), as Boring states. In this connection I should like to mention that one of my observers, an organist who has been totally blind since the age of three, has never been able to experience or to understand the attribute of volume, although it has been suggested to him. Sounds have direction, distance, timbre, loudness, brightness, and pitch; but he can not discriminate 'volume.'

Conclusion. In this paper I have written as though Boring were an avowed opponent of the resonance theory. This is not the case, for, as he himself says at the outset of his paper, "If my paper can stimulate more theorizing and more research that may eventually establish the place [*i.e.* resonance] theory, I shall be just as happy at seeing Helmholtz's sixty-year-old hypothesis vindicated at last" (p.158). He considers, however, that the facts now point to a non-resonance theory of hearing. It does not seem to me, for the reasons I have given above, that the evidence justifies the rejection of the resonance theory; but I shall be just as happy as Professor Boring should the truth of a non-resonance theory be finally established.

SELF-AWARENESS AND MEANING

A Critical Discussion of Elisabeth Amen's Experimental Study of the Self in Psychology

By Mary Whiton Calkins, Wellesley College

I. The self, accustomed to neglect at the hands of scientists, is much set up to find itself the sole topic of discussion in Dr. Elisabeth Amen's "Experimental Study of the Self in Psychology."[1] Other investigators have incidentally discovered a self in the experimental study of recognition of choice and of problem solving, but Miss Amen for the first time tackles, as the main experimental problem, the question of the self in psychology. In a brief historical introduction she summarizes first what she calls the self of scientific psychology, as a "sum total or interrelated system of . . . experiences"[2] paralleled by " an organization of the nervous system;"[3] second the self of sensationalistic psychology "which attempts to reduce the experience of self wholly or chiefly to complexes of kinaesthetic or organic sensation" (pp. 5 ff.);[4] and finally the self of self-psychology, the self experienced as conscious of itself and its environment. She rejects the first of these conceptions on the grounds that it "fails to account for the observable" difference between alleged self-consciousness and mere consciousness without reference to self and the second because it over-emphasizes the "sensory and imaginal aspects of the self-experience" (p. 13). She thereupon formulates as follows the precise object of her study. "The present investigation," she says, "was undertaken with the purpose of discovering (1) whether there is an immediate unanalyzable experience of self in Miss Calkins's sense of the term self which is observable to introspection; if so (2) what are the attributive terms which most accurately describe it, and (3) whether such consciousness of self is present in all experience, or whether it accompanies one kind of experience to a greater extent than another" (p. 14).

(i) Dr. Amen's monograph consists essentially in the close study of the experimentally controlled introspections of her 4 *Os*. Her "experiments" fall into six groups. In three of these her *Os* chose between two conventional designs—the one "which you would like to have shown to you a second time for the purpose of describing." Most of these introspections were guided by the definite question: "what differences occurred introspectively between these two experiences (a) of choice and (b) of recognition" of the chosen design when displayed for the second time (p. 19)? In Group IV of the experiments, Miss Amen substituted "for the recognition experiment a more active type of experience—the solving of a problem" (p. 41), and in Groups V-VI her *Os* dealt "entirely with the type of consciousness involved in the mental solution of a problem" (p. 46). Many of the problems were selected by the experimenter from standard tests, such as the Terman Group Test of Mental Ability, though "some were included from

[1] E. W. Amen, *Psychol. Monog.*, No. 165, 1926.

[2] J. S. Moore, *Foundations of Psychology*, 1921, 75 (Amen, p. 1).

[3] W. B. Pillsbury, *Attention*, 1908, 217-218; The ego and empirical psychology, *Philos. Rev.*, 16, 1907, 405 (Amen, p. 4).

[4] As upholders of this theory Dr. Amen names James as well as Titchener. The passages which she quotes from James certainly support her interpretation of him; but James, it must be remembered, is an arch eclectic and may equally well be quoted in support of self-psychology. Cf. for example, *Principles of Psychology*, I, Chap. IX, 226, or *Briefer Psychology*, Chap. XI, 153 for the assertion: "We have no experience . . . of a mere thought which is nobody's thought. . . . The only states of consciousness that we naturally deal with are found in personal consciousness, minds, selves, concrete particular I's and you's."

miscellaneous sources" (p. 41). In Group VI the experimenter introduced the factor of competition by reading the problems aloud to the group of 4 *Os* (pp. 58 f.). All the introspectors in Groups III-VI were asked the "direct question . . . 'Were you conscious of yourself during these experiments? When? Describe the experience' " (p. 25).

The following are typical selections from these introspections. Arabic numerals have been prefixed for convenience of reference.

OBSERVER F

(1) "I have a feeling that I was somehow vaguely conscious of myself all during the act of choice—in the sense of self-effort—that it was 'I' who was making the choice, but the experience was not in any way clear" (p. 27).

(2) "I made the selection very quickly . . . without any noticeable preceding movements or strain. I . . . was quite conscious of the fact that R was my choice—and then came something which felt very much like a sudden jump of blood pressure which merged into a momentary general bodily tension. But the decision itself seemed to consist in some sort of meaning that I can't tie up with anything else which made me aware of the fact that I was going to select R" (p. 27).

(3) "In choice I was conscious of some sort of relationship existing between the task and myself. I felt that 'I was a factor in the situation' " (p. 27).

(4) "I continued to look at R. As I was doing this, in a state of . . . irresponsible uncertainty, there gradually arose an attitude toward R which . . . resolved itself into the meaning, 'Well I may as well select this one as any.' . . . I was aware of no strain, tension, bodily movement of any kind, until the meaning became vaguely sub-vocal" (p. 27 f.).

(5) "First familiarity came without any consciousness of self so far as I am aware; then 'Yes, I saw that a moment ago,' like a flash. . . . There was no self reference sufficiently definite for me to describe; it was there I think but only as a minor part of the meaning 'I saw that a moment ago' " (p. 29).

(6) "I approached the problem with the attitude 'I must make a choice' and . . . I definitely said these same words sub-vocally. Accompanying the saying of the words there was a vague consciousness of general organic sensations. . . . But there was something which meant 'I,' beyond the sub-vocal saying of I" (p. 29 f.).

(7) "All through my hesitation there was a vague consciousness of self which once or twice popped out toward focality. Again it seemed to be basically a unitary complex of organics which helped, at least, to give the meaning 'I.' . . . When analyzed this gives mostly organics and the 'I' almost fades out but when taken as a whole, the consciousness means 'I' " (p. 29 f).

(8) "Many instances of self-reference bound up with struggling, baffle ment, effort, etc. At times there seemed to be something which was I, and this something was struggling, trying to think clearly, etc. It was all bound up with sensations of various kinds and yet it seemed quite real in itself" (p. 42).

(9) "Self-reference when I reconsidered the problem after being told it was wrong. . . . There were plenty of organics, but they seemed only to intensify the self-reference that was already there, *i.e.* they welled up after I realized what a mistake I had made. I do not say that the self-reference occurred without organic sensations, but that the latter were not noticed until after the self-reference" (p. 48).

(10) "Repeated self-references, bound up intimately with organic sensations. Often there was struggling . . . without any definite self-reference and at other times . . . the struggling, the 'sinking,'

seemed to be in part activities of what I think of as myself. In these cases it was pretty definitely 'I' who was experiencing the sinking feeling. The sinking feeling wasn't just going on—it was my sinking feeling. That at least is the way observation gave it. Possibly it is just a matter of meaning but I am not sure that I am in this case able to differentiate meaning from experience. I think one could interpret it either way one wished" (p. 48).

OBSERVER M

(11) "Conscious of self in pleasure aroused by choice. 'I' did the feeling. In recognition, 'I' did the comparing, the verbal response and the recognizing" (p. 32).

(12) "Right hand one appealed because meaning of dots on left hand one was 'spotted plague' and this meaning created a kinaesthetic upset. Self reference slightly in getting the meaning and referring it to 'me' present in consciousness at that moment" (p. 43 f.).

(13) "It was 'I' who did the figuring and regarded the answers and felt the effort of strain in attempting to get them correct. (Implicit vocal imagery.) . . . . Integration is referred to a person 'me,' who stands over and above the bundle of kinaesthetics. 'Me' and kinaesthesis in attention at same time give experience of self. Kinaesthesis alone in consciousness is merely observable imagery" (p. 49).

(14) "Verbal response. Conscious of self during problem, not as active agent, but merely as being existent and in attendance. Self did not do problem—it did itself" (p. 60).

(15) " 'Me' did the work. Self reference in keeping visual imagery of problem in my mind. When problems get harder consciousness of self always comes, then actual reference to self as entity, agent of strained attention and activity" (p. 60).

OBSERVER R

(16) "Difficulty of taking in all the data makes it [the problem] unpleasant. . . . Detect at fringe of consciousness some attitude which means, 'O let children do this.' Meanwhile interesting thought comes up, 'How I have changed.' As a boy I took delight in mental arithmetic matches. 'Fleeting recollections of school days. All that interferes with the solution. Have to pick up again the threads, but find it easier now to collect myself" (p. 44).

(17) "Mistook bit for thread and lost most of the time wondering how many threads in a screw. Had visual imagery of bits and thread. . . . The only reference to self was the recollection of comparison between myself, and others, with judgment that I was inferior mechanically but superior in other ways" (p. 50).

(18) "This seemed easy but too many fractions to carry in mind and easy to make a mistake accordingly. . . . No self-reference, though when I thought of others not getting it I hoped I would get it" (p. 62).

OBSERVER H

Even H, who to the last stoutly insists that he observes no self, seems, as Miss Amen also holds, to come perilously near the description of a self-experience, at least in his attempt to state, "what in my experience compares, I suspect, with the experiences in others which they call a self-reference."

(19) "Certain situations, however, seem more 'selfish.' For example, two major responses may block each other. . . Frequently here the path of least resistance is an habitual one, very frequently, a gestural and vocal motor language habit such as shaking the head, scowling and exclaiming incipiently, 'No that's not right!' 'Now, let's see!' "

"Frequently also there occurs a visual image of the soma's own face involving all the psychosis of one's having to stand before the gaze of one's fellows. Thus really there seems to be an illusion of a unique entity, a sort of ghost within" (pp. 55 f.).

(ii) The first conclusion which Miss Amen reaches from the study of these introspections is that "there is an immediate experience of self observable to introspection" (p. 72). Of her 4 *Os*, 3 made use "in their reports of verbal terms which seem more or less explicitly to imply a self" (p. 16). One of these, Mr. Frazier, a second-year graduate student at Harvard, reported the consciousness of self in 80% of the introspections; a second, Miss Murphy, a third-year graduate student at Radcliffe, in 67%; a third, Dr. Roback, in 14% of the introspections (p. 70). The failure of the fourth *O*, Mr. Hulin, another second-year Harvard graduate student, to report any self-experience is explained by Miss Amen as due not at all to his lack of them but to his "conviction" that such experiences "do not belong to the realm of psychological report" (p. 58).

Miss Amen's second conclusion is curiously at variance with these figures, with her own rejection (pp. 4 f., 9) of the 'scientific' and the 'sensational' conceptions of self and finally with the introspective reports of her *Os*, taken at their face value. Far from coming out squarely for self-psychology she describes her *Os*' self-experience as a "meaning-sensory-imaginal complex of the perceptual order," and unequivocally opposes it to "the immediate unanalyzable experience of self in Miss Calkins's sense of the term 'self' " (p. 72). She justifies this interpretation of the introspective records by the following statement: "The descriptive terms," she says, "used by Miss Calkins in her characterization of the self as 'persistent,' 'unique,' 'fundamental . . . to its experiences' and 'related to to its environment' are not terms in which the observers in this experiment have characterized the self-experience."

II. The preceding section of this paper has briefly summarized Miss Amen's conclusions. The following pages attempt to subject her reading of the introspections of her *Os* to that close criticism which her investigation, by its open-mindedness, its admirable method, and its lucidity throughout merits.

(i) The criticism first to be urged challenges Miss Amen's right to deny that her *Os* have "an experience of self in Miss Calkins' sense of the term." For Miss Amen inexplicably conceives, and teaches her *Os* to conceive, of this as "unanalyzable" experience, whereas Miss Calkins has consistently urged that the self, as well as the consciousness of self, is complex. Curiously enough, Miss Amen herself quotes (p. 10) from Miss Calkins an unequivocal statement to this effect: "The self though indefinable is not on this account elemental and thus indescribable." In repudiating an elemental self-experience, these *Os* are therefore in entire agreement with Miss Calkins herself. But, burdened at the outset by so heavy a misconception, it is little wonder if they do not realize the essential likeness of their position and hers.

It is next to be noted that Miss Amen somewhat overstresses the criticism, made by her *Os*, on Miss Calkins's account of the experience of self. The *Os* were asked "whether or not they could accept as descriptively true of their own experience" for "characterizations of the self-experience quoted . . . from Miss Calkins" (p. 39). Upon their replies, which may be summarized as follows, Miss Amen bases her conclusion that her *Os* fundamentally diverge from the view of self-psychology.

(a) All 4 *Os* agree with Miss Calkins in the assertion that "a feeling is either mine or somebody else's," that is, in the conception of a self, both as basal to experience and as unique.

(b) This should certainly carry with it agreement in the implied assertions: "all genuine psychic processes . . . belong to an I" and "we can not talk about experiencing without an I which experiences and which constitutes the 'essential foundation' of the experiences." And though, as a matter of fact, H and R 'reject,' while F and M 'question,' these last two statements, the disagreement furnishes a shaky foundation for positive conclusion. For Miss Amen herself regards H as a biased reporter of introspection (p. 58); R offers only 14 cases of self-reference and is notoriously "self-blind;" F and M are concerned to deny not the occurrence but the *invariable* occurrence of the experience of self as fundamental to its experiences. Their directly reported introspections attest this. "In recognition, 'I' did the . . . recognizing," M says.[5] "It was I who was making the choice," F asserts.[6] "The sinking feeling was not just going on, it was my sinking feeling," she says later.[7]

(c) All 4 *Os* also are said to reject "emphatically" the statement that "not only mental imagery but the consciousness of myself as 'the same ego then as now' is essential to recognition," yet more than one of the introspective records clearly indicates the presence of this experience by such statements as "I was conscious of myself . . . realizing that I had experienced a similar situation and . . . was reacting to it in a similar manner."[8] In the face of these records of directly introspected 'persistence' of self, it is most natural to conclude that Miss Amen's *Os* objected to the phrase 'the same ego then as now' because of some occult metaphysical meaning, which it was never intended to convey.

(d) The rejection, finally, by all 4 *Os* of "relatedness" as a fundamental character of self is a similar instance of purely verbal divergence. For by 'relation-to-environment' Miss Calkins means consciousness,[9] and all these *Os* in their introspection, tacitly or explicitly attribute consciousness to self.[10]

The preceding paragraph has already suggested that, compared with the spontaneous and concrete agreement of Miss Amen's *Os* with Miss Calkins's account of self, the criticisms on her terminology, called out by Miss Amen's questions, shrink in significance. Indeed the reported introspections fairly teem with indications of a self in Miss Calkins's actual use of the term. In Observations (14) and (15), for example, M stresses the active aspect of the self in express contrast with its receptivity;[11] in observation (16), R is interested in himself as changing or changed;[12] and in all his reported observations R makes much of that comparison between himself and his fellow-selves so strongly emphasized by the self-psychologist, particularly in the analytic study of emotion.[13]

(ii) The preceding criticisms have all concerned themselves with Miss Amen's refusal to identify her self-experience with that of the self-psychologist. But no less questionable than the negative conclusion that these *Os* really disagree with Miss Calkins in their own account of

[5]Observation (11), above. Cf. Observation (13).

[6]Observation (1), above.

[7]Observation (10), above. Cf. W. McDougall, *Outline of Psychology*, 1923, 40.

[8]Quoted from Observation (23), below. Cf. Observation (5) and Amen, p. 37, (R, Jan. 22).

[9]Cf. M. W. Calkins, *First Book in Psychology*, 1909, 3.

[10]Cf. Observation (3).

[11]Cf. on self as active, M. W. Calkins *First Book in Psychology*, 3, 11 f., 181 f., 226 ff.; also Ach, and Michotte and Prüm, quoted by Miss Calkins this JOURNAL, 26, 1915, 508-516; also A. H. Martin, Experimental study of the factors of choice," *Arch. Psychol.*, No. 51, 1922.

[12]On the self as changing, cf. Calkins, "The truly psychological behaviorism, *Psychol. Rev.*, 28, 1921, 14 f.

[13]On the significance of the consciousness of *other selves*, cf. Calkins, *First Book in Psychology*, 12, 144 f., 179, 187 ff., 251 ff.

self-awareness is Miss Amen's positive conception of their self-experience as "a complex integration of the perceptual type . . . whose characteristic feature is meaning" (p. 69). This conception is based, in the first place, on insufficient evidence. A careful study of the direct introspective reports, one hundred in number,[14] which Miss Amen quotes for the use of her readers discloses only 23 reports in which the words *meaning, mean* or *meant* occur at all. Since direct references to the self are found in 58 of these 100 reports—not to name at least 20 more of equivalent import which lack the word 'self'[15] it would seem, on the face of these reports, a hopeless task to seek in the apparently infrequent experience of meaning a clue to the far more often reported experience of self. Such a suspicion is strengthened by a closer study of the introspective reports which are marked by the occurrence of the term 'meaning.' In a third of them, or more exactly in 8 of the 23, the term 'means' or 'meant' or 'has meaning' is used in the ordinary sense of 'signifies' or 'regularly suggests' or 'indicates.'[16] Examples are:

(20) F: "What tensing of muscles meant I do not know" (p. 29).

(21) F: "The meaning of the relationship [between figures R and L] was not that L was smaller but that R was larger" (p. 28).

(22) F: "Then a complex of bodily sensations which meant 'I must make myself remember this'" (p. 17).

The first two of these quotations can not record cases of self-experience characterized by meaning, for self-experience is not so much as mentioned; in the third quotation meaning is a mere pointing to self-awareness. In at least four other passages 'meaning' is clearly used without relation to 'self-experience'—twice attributed to figures (pp. 17 and 32) and once, in Observation (11), to dots. There is, in fact, among these 23 statements only one unequivocal identification of self-reference and meaning: the assertion, in Observation (7), "a complex of organics helped to give the meaning 'I.'"

In the face of this result, it is, at first blush, hard to account for Miss Amen's very different conclusion—based to be sure not on the 100 introspections which she quotes, but on all those of her *Os*' reports (149 of the total 313) which she regards as containing self-reference. She tells us that meaning was a component of self-experience in nearly 100% of F's 59 reported cases of self-experience (p. 64) and in over 80% of R's 14 cases (p. 68), though she points out also that M reports meaning in much under 1% of her 76 self-experiences (p. 67). With her usual candor, Miss Amen herself supplies the explanation of the glaring disproportion between her result and that of the preceding paragraph. She has attributed 'meaning' to experiences which do not claim it for themselves. Thus she says (p. 65) that "the experimenter has classified as meaning such statements of F as the following:

(23) "I was conscious of myself in the sense of realizing that *I had experienced a similar situation before, and that I was reacting to it in a similar manner.* And, when I thought, 'I may as well choose one as another,' I thought of myself in the sense *that I had no preference* and *that my choice would have no consequences for myself.*"

(24) "I was at times conscious *that 'I' must make this selection; that there was an obligation on my part.* This was not definitely formulated, but this feeling *that 'I' was responsible for getting this done* came to me on two or three occasions."[17]

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[14]By the writer's count.

[15]Cf. for example, Observation (18), above. In many of these 20 cases the term 'self' is replaced by 'I' or by 'me,' often italicised.

[16]Cf. Amen, *op. cit.*, 17, 27, 28, 29, 30, 47, 49, 60, 28.

[17]Miss Amen's only other example of what she calls meaning—"There was something which meant 'I,'" beyond the sub-vocal saying of 'I'—is another case of meaning in the neutral sense of 'signifying.'

But Miss Amen certainly is not justified in the assumption that these are cases of meaning. Any self-psychologist would unhesitatingly claim both quoted passages as typical statements of a self's incompletely analyzed experience—the first including a reference to the persisting or enduring self with its recurring experiences, the second stressing the individuality of self.

Miss Amen supplements this dubious interpretation of direct introspective records by appeal to the 'generalized descriptions of the self-experience' which she asked from F, M, and R (p. 53). F's statement follows, only slightly abbreviated:

(25) "I can not tell whether what I have been referring to as 'I' is really something which is directly experienced or whether it is meaning. Usually it appears to be the latter. . .

"When I say that such and such an experience carries the meaning of being uniquely personal—of being not the experience of Tom Smith but of the individual who I am. . . .

". . . Surely, taken on one level one would say, 'Here is an entity of some kind, something unique and real and active.' So says common sense. . . However, it seems far more probable that what I have denoted as 'I' is not a self but a consciousness of 'selfness,' . . . in other words that the 'I' is the meaning which has come to be attached to certain experiential processes.

"But this is guess work. I think that I experience self-reference and self-activity and, when taken on an uncritical, unanalytical level, I certainly do; but the explanation of self as meaning is so reasonable that one has a tendency to consider the first as mere naïveté. It is for *E* to decide which way she wishes to look at the matter."

Here unquestionably (save in the fourth sentence) naïve self-experience is opposed, and, in certain paragraphs, reduced to meaning; but no light is shed on the connotation of the term meaning, and F very obviously hesitates to abandon the interpretation of his experience as self-reference and self-activity for the more conventional conception of it as meaning. His appeal to the experimenter to interpret for him his own introspection seriously impairs the value of his opinion.

R does not share in these vacillations. He says unequivocally that:

(26) "The self is a meaning carried in the verbal image 'I,' 'me,' 'my.' "

M, on the contrary, in her first generalized description, does not allude to meaning. For her:

(27) "Self-reference . . . consists in consciously bringing one's self into relation with the problem in question" (p. 54).

After reading F's generalized description M, to be sure, grudgingly admits that the self-experience may "really consist of meaning," but she protests to the end that she experiences "self-reference and self working on problems even;" she deprecatingly adds, "if it is to consider the problem in a naïve way" (p. 57).

The main outcome of the 'generalized descriptions' by F and M is thus the hesitating interpretation by two *O*s that what they have already many times reported to be direct self-experience may be reducible to "meaning, sensory, and imaginal components." This interpretation certainly falls far short of carrying conviction; and R does not greatly strengthen the case for the meaning theory. Only 14 cases of self-reference are attributed to him; and he does not once use the word 'meaning' in any quoted introspection.

Even more serious than failure to prove that her *O*s really do conceive their reported self-experience as meaning is the utter uncertainty and vagueness of Miss Amen's use of the term. She everywhere assumes either that the connotation of the word is fixed or else that the divergences in its use are negligible. "Whatever the particular conception of meaning

implied in these reports, meaning," she says, "is acknowledged by all four observers to be an aspect of experience which is reportable, which is a part content of the consciousness under introspection, and which is therefore experiential" (p. 38). This indifference to the signification of the term meaning is wholly unjustified, for as a matter of fact, in some conceptions of it, 'meaning' can not possibly be interpreted as a distinctive component of self-experience; and, in other conceptions, meaning is from the start merely a synonym for self-experiences.

(a) Under the first head, fall the repeated instances already referred to of meaning in its original sense of 'signifying,' 'pointing to,' 'suggesting.' That which is pointed to is sometimes, though not always, the self. But to say, (28) "The meaning of the whole experience was that this success was mine" (p. 48) no more signifies that the suggested self-experience essentially *consists in* meaning than the words, "the meaning of the dots was spotted plague"[18] signifies that spotted plague consists in meaning. In both cases meaning is simply the function of 'pointing to' or 'suggesting' something. Similarly, to assert that 'sensations' (p. 49) or 'tension' (p. 27) or 'something' (p. 47) *meant* self is far from saying that meaning is distinctive of self-experience. In this group belong also cases in which 'meaning,' used now in the sense of imaginal context, is indeed referred to self but not identified with self. An example is:

(29) "The meaning of each action appeared in the imaginal consequences as referred to *myself*" (p. 47).

(b) Besides these cases of the use of 'meaning' in a perfectly neutral sense there certainly occur repeated instances in which self-experience, or some aspect of it, is described by Miss Amen's *O*s as 'meaning.' Examples occur in Observations 2, 4, 5, 7 and 10, already noted. Miss Amen treats none of these cases in a fashion to justify her conclusion, for she really gives no glimmering of a notion of what she means by 'meaning.' If, as sometimes appears, she is employing the term in what Ogden and Richards call the "traditional and mystery sense" she is exposing herself to the judgment which she metes out to the conception of elemental self-awareness (pp. 69, 72). If she is falling back on the use of meaning as imaginal context, she is really reverting to the rejected concept of the scientific self. We can only guess that Miss Amen virtually uses 'meaning' in Titchener's sense of unscientific awareness of "the world of things and people."[19] In this case, however, meaning is merely a verbal synonym of the term self-experience; and the conclusion, alleged self-experience is a complex distinguished by meaning, reduces to sheer tautology.

In brief, Miss Amen's account of her *O*s' self-experience as essentially meaning is unsatisfactory first, because it is so insufficiently borne out by their direct introspections; second, because she never clearly tells us what she means by meaning; third, because many of the senses of the hydra-headed word meaning can not possibly serve as clues to the self-experience; and, finally, because the word meaning often is used in such a way as clearly to imply the very sort of self-experience which Miss Amen denies.

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[18]Cf. Observation (12).

[19]Description *vs.* statement of meaning, this JOURNAL, 23, 1912, 167. For Titchener's more frequent use of the term as 'imaginal context,' cf. *Experimental Psychology of the Thought Processes*, 1909, 175 ff.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

LVI. The Moore Tests of Radical and Conservative Temperaments

By M. F. Washburn, H. Kepler, N. McBroom, W. Pritchard, and I. Reimer

At the 1924 meeting of the American Psychological Association, in a joint session with the American Sociological Association, Professor H. T. Moore presented a paper entitled, "Innate Factors in Radicalism and Conservatism."[1] It was based upon questionary returns from 163 Yale undergraduates and 163 Dartmouth undergraduates, and upon experimental results from two groups of students, selected by means of the questionary from a class of 225, presumably Dartmouth undergraduates. The questionary consisted of 20 questions to which the *S* was required to answer 'Yes' or 'No;' according to the nature of his answer his opinions were judged to be radical or conservative. The questions related to the entrance of the United States into the League of Nations, to its general policy of isolation from European affairs, to sympathy with the present Russian government, to pacifism, to the idea of a Superstate, to government ownership of railroads, to limitation of the power of the Supreme Court, to the formation of a Labor Party, to higher taxation of the wealthy class, to direct popular vote on governmental matters, to trade unionism, to a minimum wage, to coöperation of employees in management, to the influence of Wall Street in politics, to the relative influence of opportunity and ability in determining a man's industrial class, to the dangers and advantages of race prejudice, to the theory of evolution, to the relative weight of tradition and present usefulness in determining support of an institution, to the alleviation of poverty by social legislation, and to promiscuity in sex life on the part of unmarried persons. After this questionary had been tried out on the Yale and Dartmouth groups, it was given to a class of 225 students: those men who gave 'radical responses' to 14 or more of the questions were classed as radicals, and those who gave radical responses to not more than 7 questions were classed as conservatives. Two groups of 30 observers each were thus obtained, and were then subjected to certain experimental tests in the endeavor to see whether the test results would separate the groups.

The results were so clearly cut and so interesting that we were incited to repeat some of the tests with Vassar students. The questionary was accordingly given to 174 of these. The twentieth question was omitted, because of the probability that its being propounded to young women college students would be pounced upon by a certain class of newspapers as news of the most acceptable and highly flavored type. Since the popular opinion is that women are more conservative than men, it is of some interest to note that in the sum of percentages of radical responses to the 19 questions used Vassar, with 964, stands between Yale, with 904, and Dartmouth, with 1052. (The figures for Yale and Dartmouth are estimated from the graphs.) To five questions Vassar gave more radical responses than did Yale, and to only five notably fewer; only on the second question, relating to American isolation from European affairs, did Vassar exceed Dartmouth in percentage of radical responses. Ninety-three percent of the Vassar group expressed themselves against isolation: it is possible that in the two years elapsing between the answers of the men and

[1] H. T. Moore, *J. Abn. Psychol. & Soc. Psychol.*, 20, 1925, 234-244.

those of the women there has been a shift of public opinion in this direction. Only about 76% of the Yale men and 70% of the Dartmouth men gave the radical answer to the question.

Our chief interest lay in securing radical and conservative groups for testing. We found that we could not use Moore's criteria of fourteen radical answers as the lower limit of radicalism and seven radical answers as the lower limit of conservatism, for two equal groups could not be constructed at Vassar on this basis. It was impossible to find enough conservatives. There was one case of 3 radical answers, one of 4, three of 5, seven of 6, nine of 7, making a total of only 21 cases where the number of radical answers was 7 or less. On the other hand, there were eight cases of 18 radical answers, three of 17, fifteen of 16, twelve of 15, twenty-eight of 14, making a total of 66 cases where the number of radical answers was 14 or more. We are at a loss to explain why we had so few extreme conservatives as compared with the Dartmouth class of 225, when the group of 163 Dartmouth men made a considerably higher percentage of radical responses than did our Vassar group of 174.[2]

Moore's two groups contained 30 observers each. We shall present results from a radical section of 26 young women, comprising all those who made 16 or more radical responses, and a conservative section of 21, comprising all those who made 7 or fewer radical responses; also from two sections of 66 members each, comprising respectively those who gave 14 or more radical answers and those who gave 9 or fewer.

Professor Moore's first concern was to compare the intellectual ability of his two groups. Accordingly we secured information as to the percentile rank in her freshman test (the Thorndike Tests) of each student on our lists. The average percentile rank of the conservatives was 49 that of the radicals 44; in the conservative group 47% were better than the radical average, while in the radical group 50% were better than the conservative average. Thus, as in Moore's investigation, there was no significant difference in general mental ability.

Mirror-Drawing Test. The first experimental test performed in the Dartmouth research was the Mirror-Drawing Test. We made this test upon the *Ss* in our two groups, following the directions in Whipple's Manual of Physical and Mental Tests, and recording for each *S* the average time for tracing five stars, the average number of errors, and a "total efficiency score computed by multiplying seconds by errors," as Moore says he did. He does not, however, state whether his *Ss* were scored on one star only, or on more. His results were as follows: "On the score for accuracy 81% of the radicals made fewer errors than the average conservative and only 25% of the conservatives made fewer errors than the average radicals." In our experiments, on the other hand, using averages of errors in the tracing of five stars, only 51% of the radicals made fewer errors than the average conservative, and 65% of the conservatives made fewer errors than the average radical. When the errors only of the first trial at tracing the star are considered, 66% of the radicals did better than the average conservative, and 52% of the conservatives better than the average radical. "On the score for speed," Moore continues, "the difference is not marked, but in gross efficiency 91.3% of the radicals surpassed the average conservative." In our results from five tracings, 81% of the conservatives did better than the average radical, and 74% of the radicals better than the average conservative. The two averages were very close together, 3444 for the conservatives and 3393 for the radicals. The peculiar distribution of percentages is due to the wide range of scores, which was for the conservatives from 459 to 22514, and for the radicals from 308 to 21604.

[2]Our omission of question 20 cannot explain it, for on this question the Dartmouth group of 163 gave an equal number of radical and conservative answers. No figures are reported on this point for the group of 225.

When the efficiencies of only the first trials are considered, 81% of the radicals surpass the conservative average and 79% of the conservatives surpass the radical average: the conservative average is 10176 and the radical average 9169, which means a very slight superiority for the radicals. In general, however, it is clear that we do not find the radicals significantly better than the conservatives as regards mirror-drawing. Moore interprets the superiority which he finds as meaning that the conservative temperament finds old established habits harder to break than does the radical temperament.

We also undertook to compare the larger groups. If the results from all those *Ss* who gave 14 or more radical answers were compared with the results from those *Ss* who gave 9 or fewer radical answers, the radical and conservative groups had 66 members each, including, of course, in the conservative group less extreme conservatives than was the case in the smaller conservative group, and in the radical group less extreme radicals. Of the radical group 59.7% surpassed the conservative group in efficiency, while 71.6% of the conservatives surpassed the radicals.

It may be noted that another factor besides the capacity to break habits readily is involved in success with the Mirror Drawing test: namely, the tendency not to be easily flurried. What makes the errors pile up is a panicky condition: phlegmatic temperaments ought, it would seem, to do better on this account, and if we accept the common view, the conservative is more phlegmatic than the radical.

Reaction Test. Professor Moore next determined the mean reaction-times of his two groups and found that the mean time of the radicals was 131.7 σ and that of the conservatives 154 σ, and that 91.3% of the radicals were quicker than the average conservative, while 27.3% of the conservatives were quicker than the average radical. He does not state how many reactions were made by each *S*.

We took for each of our *Ss* two series of 10 reactions each, using the Sanford vernier chronoscope. In the case of our two smaller groups, comprising those who made 16 or more radical responses and those who made 7 or fewer, for the first series of 10 reactions the average time of the radicals was 156 σ and that of the conservatives 166 σ: 73% of the radicals were quicker than the average conservative, and 45% of the conservatives quicker than the average radical. For the second series of 10 reactions the average time of the radicals was 140 σ and that of the conservatives 149 σ: 69% of the radicals were quicker than the average conservative and 40% of the conservatives quicker than the average radical.

When the larger groups of 66 (less extreme) radicals and conservatives respectively were compared, the average times for the 20 reactions were 156 σ in the case of the radicals and 157 σ in the case of the conservatives: 58.2% of each group surpassed the average of the other group in speed.

It thus appears that there is only a very slight superiority, if any, of radicals over conservatives in the matter of reaction time.

Card Sorting Test. Another test used by Moore was "one devised by Münsterberg. The subject is asked to sort a special kind of pack of cards under conditions in which both speed and accuracy are important. What is especially significant for the purpose of this study is that the subject does not know the relative importance of the two factors. . . . Now under these conditions 70.9% of the radicals work faster than the average conservative. In other words, when the relative importance of accuracy is uncertain the radical more naturally inclines to quick snap judgments, while the fear of taking chances is sufficiently ingrained in the conservative to find its way into his behavior even in a psychological laboratory."

To repeat this test as well as we could considering the rather vague description of it given by Moore, we used the pack of cards supplied by Stoelting for the Card Sorting Test, being careful always to use the sam

formula of instruction, which emphasized equally speed and accuracy. For each *S* the average of five successive sortings was taken. With our smaller groups, comprised of those who made 16 or more and those who made 7 or fewer radical responses, the average time for the radical group was 92.5 sec., and that for the conservative group 77 sec. The percentage of radicals who were quicker than the conservative average was 36; that of the conservatives who were quicker than the radical average was 94. The average number of errors made by the radicals was .53; by the conservatives, .56. There is thus no trace of a tendency on the part of the radicals to prefer speed to accuracy.

In the larger groups, of 66 *S*s each, the greater speed of the conservatives disappears, and the percentage of radicals working faster than the conservative average is 71.6, close to Moore's result. But the radicals are also somewhat more accurate: 67.1% of them averaged fewer errors than the conservative average. Moore gives no figures with regard to errors which justify his statement that the radicals preferred speed to accuracy.

It is probable that Moore like us used the average of five card-sortings. But in case he made only one, we may note that the average time required for the first sorting by our smaller radical group was 105 sec., and their average number of errors in the first sorting .86; while the average time for the small conservative group was 94 sec. and their average number of errors 1.2. In this case it was the conservatives who sacrificed accuracy to speed.

Free Association Test. "As a last experiment each man was given the 100 words of the standard Kent-Rosanoff list,[3] and was asked to write down the first word called to mind by the stimulus word. In scoring the papers every word having a frequency value of less than 10 in the Kent-Rosanoff tables was listed as an unusual association. The average number of unusual words for the conservative was 29, and for the radicals 38.2; but the mean deviation was larger than the difference in the means, so that we have here at best a mere suggestion as to greater originality, or eccentricity in the radical's simplest association of ideas."

For our smaller groups, the average number of unusual words was in the case of the conservatives 32.2, and in that of the radicals 37.4. In the larger groups the difference became negligible; the figures were 34.14 for the conservatives and 35.38 for the radicals. There is thus no tendency for the radicals to prefer unusual associations.

Hence it appears that our results throughout fail to confirm Moore's. The radicals do not tend to learn mirror-drawing noticeably faster, to have noticeably shorter reaction-times, to sacrifice accuracy to speed in card sorting, or to give more unusual responses in free association. This outcome of our work was a disappointment to us. The way of the deviser of tests for temperamental differences is indeed hard and discouraging. Is the fault perhaps, in the present instance, with the questionary? Is a radical temperament necessarily indicated by radical opinions on public questions? Is there a sex difference by virtue of which the opinions of women on public questions are less indicative of temperament than those of men?

[3] For this list see this JOURNAL, 25, 1914, 592.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF STANFORD UNIVERSITY

II. The Rating of Psychologists

By Miles A. Tinker, B. D. Thuma, and Paul R. Farnsworth

Graduate students are interested in the relative importance of the older psychologists, philosophers and the like, as background for present-day orientations. With this fact in view, the writers prepared a questionary in order to gain an insight into current opinion. The data so gathered suffer from at least the following ills.

(1) An absolute scale, showing the importance of historical figures, can not be constructed. Importance is a relative matter and changes from age to age, and from one school of thought to another.

(2) Ratings from England, France, or Germany might possibly give quite different results. The present scale shows only the composite judgment of seventy-three American psychologists who hold the higher scholastic (and allied) positions in this country.

(3) There is always the danger of lack of coöperation in filling out questionaries. The writers hope, however, that they have passed the test laid down by Boring.[1] In general they feel that they have received excellent coöperation.

(4) The scale omits names which many psychologists will feel of prime importance. Space was left on the questionary for such additions, but the names added showed practically no agreement and few appeared more than once. This rating list, then, merely shows the rank and order of importance of historical personages arbitrarily selected by the writers and rated by seventy-three prominent American psychologists. The names selected for rating were those of psychologists (and others whose work affected psychology) who were either at present deceased, or were foreigners whose psychological reputations were made some years ago.

The questionary was as follows:

"We are interested in ascertaining the relative importance of those men who have contributed to the history of general psychology. We have in mind their importance from the standpoint of the student obtaining his doctorate in psychology.

"Please rate the following individuals on a scale of four: If the knowledge of an individual's contribution to psychology is extremely important, place (1) before his name; if quite important, but not of first importance, place (2) before his name; if of comparatively little importance, but still worthy of mention, place (3) before his name; if of no importance, place (4) before his name. Do not rate those names which are unfamiliar to you. If there are names not appearing on this list, which are important in your opinion, kindly note them on the back of this page with their respective ranks."

The names of the individuals to be rated were then listed in alphabetic order.

A scoring device suggested by Dr. T. L. Kelley was adopted. Rating 1 was given the value of 1, 2 of 2, 3 of 3, 4 of 4, and 0 (omission) of 4. Mean scores were figured for each name on the questionary, and a grand mean average for all. The names were then placed in two piles—the first for those whose means were above the grand mean, and the second for those whose means were below. The percentage of 'one' ratings was next figured

[1] E. G. Boring, this Journal 37, 1926, 632.

for each of the two groups. The same was done for the 'twos,' 'threes,' 'fours', and 'zeros' (omissions). The percentages of ratings above and below the mean, are as follows.

Rating :	1	2	3	4	0
Above :	94.5%	76.0%	45.5%	15.0%	13.6%
Mean :					
Below :	05.5%	24.0%	54.5%	85.0%	86.4%

Using the Kelley formulae for p and x, given in Appendix C of his *Statistical Method*, the ratings have the following values: 1 = 1.60; 2 = .71; 3 = −.11; 4 = −1.04; 0 = −1.10. Changing to positive whole numbers these become approximately: 1 = 4; 2 = 3; 3 = 2; 4 = 1; 0 = 1. The writers have adopted the inverse of these and have given the ratings their face value (4 = 4; 3 = 3; 2 = 2; 1 = 1;) excepting the 0, which thus gets the same value as 4.

The ranking of the men included in the list, as obtained by averaging the answers from 73 psychologists is given in the Table. It is interesting to note that in only one instance was the rating curve for a subject bimodal.

TABLE: RESULTS OF THE RATINGS

Rating	Order of Merit	Name	No. of Zeros	Rating	Order of Merit	Name	No. of Zeros
1.05	1	W. James	0	2.41	34.5	Kant	5
1.11	2	W. Wundt	0	2.44	36	Romanes	2
1.23	3	Binet	0	2.45	37.5	Lotze	5
1.32	4	Helmholtz	0	2.45	37.5	Spinoza	8
1.37	5	Ebbinghaus	1	2.53	39.5	Höffding	1
1.41	6	Fechner	1	2.53	39.5	Sanford	1
1.44	7	G. S. Hall	0	2.55	41	Lange	11
1.51	8	Galton	2	2.56	42	Hobbes	6
1.68	9.5	Herbart	1	2.63	43	Ach	13
1.68	9.5	Ch. Darwin	0	2.64	44	Hartley	11
1.71	11	Weber	3	2.66	45	Purkinje	7
1.74	12	Stumpf	0	2.68	46	Leibnitz	7
1.75	13.5	Locke	2	2.70	47	Marbe	11
1.75	13.5	Aristotle	2	2.73	48	J. Müller	22
1.77	15	O. Külpe	0	2.74	49	Bergson	2
1.81	16	Hering	2	2.77	50	Comte	5
1.84	17	H. Spencer	2	2.78	51	Royce	3
1.88	18	Ribot	1	2.81	53	Lombroso	5
1.92	19.5	Münsterberg	1	2.81	53	Nagel	18
1.92	19.5	Berkeley	1	2.81	53	Broca	11
2.01	21	Hume	4	2.84	55	J. Mill	13
2.04	22.5	A. Bain	0	2.85	56	Rousseau	7
2.04	22.5	Descartes	1	2.92	57	Condillac	14
2.11	24	G. E. Müller	9	2.93	58.5	Hartmann	16
2.22	25	Charcot	4	2.93	58.5	Meinong	18
2.27	26	Plato	2	3.00	60	Thomas Aquinas	10
2.28	27	J. Ward	4	3.01	61	F. Bacon	9
2.33	28	Mach	7	3.03	62	Socrates	11
2.34	29	Lipps	6	3.05	63	Lamarck	13
2.36	30	Loeb	7	3.11	64	Wernicke	24
2.37	31.5	Head	8	3.14	65	Flournoy	19
2.37	31.5	Brentano	9	3.15	66	Holmgren	17
2.40	33	J. S. Mill	2	3.18	67	Fichte	11
2.41	34.5	Mosso	5	3.21	68.5	Delboeuf	26

TABLE (continued)

Rating	Order of Merit	Name	No. of Zeros
3.21	68.5	Wolff	22
3.22	70	St. Augustine	15
3.25	72	Hegel	8
3.25	72	Schopenhauer	9
3.25	72	Vierordt	22
3.26	74	Carpenter	24
3.30	75	M. Müller	25
3.32	76.5	Volkmann	25
3.32	76.5	Avenarius	24
3.33	78.5	Democritus	15
3.33	78.5	Plotinus	20
3.35	80	Malebranche	17
3.36	81	Roger Bacon	17
3.37	82.5	Galen	22
3.37	82.5	Epicurus	13
3.42	84	Beneke	28
3.47	85	Bell	35
3.52	86	Drobisch	35
3.55	87	Pythagoras	19
3.57	88	Bonnet	35
3.58	89	Empedocles	24
3.62	90	Tetens	35
3.66	91	Schelling	22
3.67	93	Protagoras	24
3.67	93	Anaxagoras	20
3.67	93	Zeno the Stoic	20
3.68	95	Diderot	28
3.70	97	Averröes	40
3.70	97	La Mettrie	39
3.70	97	Waitz	46
3.73	100	Abelard	22
3.73	100	Erasmus Darwin	27
3.73	100	Magendie	43
3.74	103	Anaximander	24
3.74	103	Jacobi	42
3.74	103	Boehme	41
3.75	105	Herder	30
3.77	107.5	Parmenides	33
3.77	107.5	Duns Scotus	30
3.77	107.5	Schleiermacher	25
3.77	107.5	Theophrastus	41
3.78	110	Albertus Magnus	33
3.79	111	Gassendi	42
3.82	112	Anaximenes	29
3.86	114	Alhacen	51
3.86	114	Meister Eckhart	44
3.86	114	Xenophanes	31
3.88	116.5	Origen	40
3.88	116.5	Paracelsus	38
3.89	118	Ptolemy	26
3.93	119	Campanella	52

This exception occurred with Plato, who was rated very high by one group and very low by another. The correlations by the rank method between rank order of merit and number of zero ratings was 0.93: paucity of ratings for any given psychologist and low rating appear together. A majority of the names appearing in the two upper quartiles are those which have meant much to experimental psychology while the two lower quartiles are made up largely of philosophers.

CONCLUSIONS

(1) The questionary was developed to aid the graduate student in ascertaining the concensus of opinion concerning the relative importance of persons contributing to the development of psychology.

(2) With a few exceptions, the philosophers, to whom many of the present day historians of psychology devote most of their attention, received average or low ratings.

(3) All ratings were distinctly unimodal except that of Plato.

(4) A correlation of 0.93 (rank method) was found between rank order of merit and number of zero ratings (absence of ratings).[2]

[2]The writers are grateful to Professors L. M. Terman and Walter R. Miles for affixing their names to the questionary, for it otherwise might not have obtained the consideration it received. Dr. Terman also arranged for the financing of the study.

BOOK REVIEWS

Outline of Abnormal Psychology. By W. McDougall. New York, Charles Scribner's Sons, 1926, pp. xvi, 571.

The reviewer has approached McDougall's *Outline of Abnormal Psychology* with great interest and lively hopes. The publishers, in their circular had described the book as "of the highest significance to the medical profession," adding that McDougall's preparation for this work has been "undeniably unique," instancing his thirty years of prominence in "the academic study of psychology and in the leading American and English organizations for psychic research," and his excellent opportunities for the study of mental abnormalities during the war. With nice discrimination, the publishers have not claimed that the book has any specific importance for the psychologist; but nevertheless, the reviewer, realizing the opportunities that the author has actually enjoyed, was really disappointed at finding the character of the animal which the labor of the mountain has produced.

As material for the study of McDougall's own psychological development, the book is indeed interesting. But as a text for students, it is impossible to see how it can be otherwise than confusing and obstructing. The book is in fact, astounding. It is devoted to the magnification of three men; McDougall, Freud and Jung; but while much space, (with copious quotations from his other writings), is devoted to the justification of McDougall's doctrines, every doctrine of Freud and Jung is denounced, with such expressions as "the wild and untrue generalizations which we meet on nearly every page of Freudian literature" (132), or refuted with subtler words. Contempt is poured also on the lesser psychoanalysts.

Were it not for the well-known seriousness and solemnity of McDougall's method, we might well think the volume an ironical assault on the whole aggregation of psychoanalysts. While announcing flatteringly that: "I believe that Prof. Freud has done more for the advancement of psychology than any student since Aristotle" (viii); "I have enjoyed the great advantage of having my dreams analysed by Dr. C. G. Jung" (xi); "I have the greatest respect and admiration for Dr. Jung's work and person" (150); and performing many other profound acts of worship, all strikingly dragged in; McDougall completes the most devastating analysis ever made of the psychoanalytic doctrines. My own brief assult on Freudianism is a paean of praise in comparison. The progressive renunciation by Freud of his wonderful discoveries; the rejection of the important Freudian principles by his most serious and able disciplies; the utter confusion of conflicting doctrines among the psychoanalysts; the stupid arrogance with which skepticism in regard to a wonderful psychoanalytic discovery is assumed to demonstrate the ignorance of the skeptic, (although the psychoanalyst himself abandons it later, and other equally great psychoanalysts have scoffed at it from the beginning); are pointed out in detail. "Obstinate skepticism" is expressed in regard to some of the most sacred psychoanalytic doctrines. Others are scored as "flying straight in the face of common sense." There are even sprinkled in some innuendoes, such as the "dream" remark: "He should learn psychoanalysis and he could pick up a few guineas and keep the whole family in comfort" (151).

But McDougall is not a joker. We have to assume that he is serious in his demolition of the 57 varities of psychoanalysis, and also serious in his emotional attitude (or is it the "submissive instinct"?) towards Freud and Jung. He must be rated equally sincere in his proclamations that Freud has made contributions of immense value, and in his showing that every specific contribution is junk. Very interesting psychoanalyses can be made of

McDougall's attitude from the standpoint of Freud's original sex theories, from the standpoint of Jung's older mother complex and newer "psychological types," and from the standpoint of Alder's inferiority complex. But these analyses would be merely scurrilous. Let us depend upon actual psychology.

McDougall has felt keenly the criticism passed on his theories. This is evidenced by his constantly recurring defense of his instincts, and his denunciations of "mechanism." (Whatever mechanism really is, it is for McDougall the combined opposition to his views.) In this book he emphasizes his standing as a *medical* psychologist and obviously lumps the contemner's of his theories as academic. He has misunderstood the criticism directed against him, and feels driven from the camp of the academics into that of the medicos, and wishes to emphasize the greatness and honor of his crowd, in spite of some difficulty in his acceptance of them. This is by no means an 'unconscious' desire or purpose.

On the other hand, there is a real affiliation between McDougall's position and that of the psychoanalysts. Jung accepts one instinct where McDougall accepts several; but they are, alike (the "libido" and the "instincts"), wellsprings of mystic energy. Whereas McDougall in certain pages denies the "unconscious mind," in other places he accepts it, under the different name of "subconscious." Moreover, in freedom of generalization, facile adaptation of data to illustrate theories, and fluidity of terms, his methods are strongly psychoanalytic. Thus, after insisting on the importance of purposes, as psychologically conceived, he proceeds to declare that the purposes may "operate wholly on the unconscious plane" (38), and that "motivations" (233) and "aversions" (251)—both of which are probably other names for 'purposes'—may be "subconscious." At times the "subconscious" is identified with Morton Prince's "coconscious" (254), but not consistently. In many cases, "subconscious" means precisely what is often meant by the Freudian "unconscious," namely, what psychologists mean by *conscious*, but where something is not reflected upon, or where the reasons for the occurrence of the conscious process are not recognized.

Throughout the volume, as has already been indicated, there is a recurrent antagonism to the theory of "ideas" as entities (a theory which probably none but the psychoanalysts hold), and to a dreadful theory called "mechanism." Yet here, as in previous writings, McDougall absolutely refrains from committing himself to any theory which is not as truly mechanistic as those of the "crude radicals." That the "instincts" and "purposes" operate in a non-casual way; that "psychic energy" comes out of nothing; that natural law does not hold in the mental realm; these anti-mechanistic doctrines *may* be held by McDougall; but he nowhere admits them. Actually, McDougall's psychology is as strictly mechanistic as that of Freud—he does not mention the widely heralded 'greatest contribution' of Freud *i.e.* the 'discovery' that mental life is as rigidly ruled by cause and effect as is the world of physical phenomena. The mechanism of Freud and McDougall really differs from that of the 'regular' (McDougal would say "crude") psychologist as the mechanism of the auto driver who refuses to inquire what goes on under the engine hood, but conceives merely of a mysterious "horsepower" driving his car, differs from the mechanism of the expert who is interested in every detail of the working of the engine.

So far as the understanding of the problems of abnormal psychology go, McDougall offers the student little help. When we abstract from the boosting of McDougall's theories, the adulation of Freud and Jung, the tearing of their theories to pieces, and the sneering at the crude psychologists, there is little left. Even in the discussion of hypnosis, where McDougall might have been expected to make good use of his technical ex-

perience, the exposition suffers from the ignoring of methods which do not come under his classification (the method of gentle persuasion, for example); and no light is thrown upon the nature of the processes involved: but, of course, this illumination could be given only by a descent to the level of the "crude" psychology which has shed what little light had fallen on hypnosis. Again, no attempt is made to connect the phenomena of the neuroses with the well understood phenomena of normal life. No use is made of the principles of habit; not even of the fact that the conscious responses of the present depend, in normal, and probably in abnormal life, on the responses of the past *whether those responses are remembered or not.* But then, this too, would involve a descent into "mechanistic" psychology; and habit is peculiarly anathema to McDougall.

Strangely enough, the reviewer is of the opinion that the Outline of Abnormal Psychology is a useful book, quite aside from its valedictorian significance. Like the vagaries of the magnetists, the Couéists, and psychoanalysts, it points definitely to the conclusion that the confused fields of abnormal psychology will not be cleared up until the painfully gathered data, and the laboriously careful methods of scientific psychology are applied to it, and applied by 'crude' and mechanistic' psychologists. Mythology after mythology has been applied—and has passed into innocuous desuetude. Psychoanalysis has passed, in so far as any scientific interest goes; let us give McDougall credit for applying the final blows. Of course it lingers in practice on account of its great financial profitableness. Scientific psychology, *i.e.* academic psychology, already has a vast fund of material to apply to the problems when it can obtain the chance, now denied it, to apply it. Medical psychology, when it becomes a reality, will be as thoroughly founded on 'academic' psychology as medical biology must be on 'academic' biology.

Johns Hopkins University KNIGHT DUNLAP

Social Psychology. By KNIGHT DUNLAP. Baltimore, Williams & Wilkins Co., 1925, pp. 261.

Professor Dunlap's treatment of social psychology in this volume is somewhat of a departure from that of other writers on this subject, although there has been little enough agreement heretofore as to its proper field. Much of the material he presents has generally been considered sociological or anthropological rather than psychological. At times the normative nature of his discussions seems to take him over into the field of social ethics. His method is to consider various phenomena of social life and organization from a psychological point of view, and to interpret them as far as possible in the light of psychological principles. There are chapters on sex differences, marriage and the family, civic, martial and religious organization, social progress, social organization and propaganda.

Social psychology is based, according to Dunlap, not on any single principle or phenomenon of mind, such as sympathy, imitation, or instinct, but on the total results of general psychology. Mind being "the sum total of the conscious responses or reactions of the individual," and psychology "the study of the conscious functions of the organism," social psychology is defined as "the study of whatever stimuli from other persons have contributed to our conscious lives, and to the activity which results from conscious life."

The author definitely rejects the concept of instincts, which are, he says, coming to be viewed merely as convenient logical classifications of behavior and not as explanatory factors or ultimate psychological or biological entities. The place accorded to instincts in some of the older treatments, and to 'drives' by other writers is largely taken in Dunlap's scheme by the 'desires.' The desires are regarded as organic conditions, outside of

the nervous system, but capable of stimulating receptors, and therefore producing and modifying conscious reactions. Provisionally the desires are classified under the heads of alimentary and excretory desires: desires for rest, activity, shelter, conformity, preëminence, for progeny, and for sex gratification.

A social mind is held to exist only in the sense of the enumerative total of individual minds, or in the sense of an individual mind considered as typical of all in the group; in either case it involves social consciousness, or, in the individual, consciousness of being a member of the group. Social feeling and social sentiment are also important factors in the social mind.

The problem of individual differences, those of race, stock and class, comes in for discussion, followed by a full chapter on sex differences, anatomical, physiological and psychological. He emphasizes especially the types of sexual desire, emotion and sensitivity, and the differences between men and women with respect to them.

Several chapters are given over to various types of social organization. The author gives an interesting and valuable discussion of marriage and the family, largely from an historical and comparative point of view. His treatment of the origin of exogamy and incest taboos is particularly enlightening. In the chapter on religious organization he touches on the nature and origin of religion, its relation to magic, the difference between personal and social religion, the relation of the desires to religion and religious needs, and the social value of religion. The various types of civic and martial organization are discussed from the point of view of classification, function, and relation to the individual.

Social progress as dependent on heredity and training leads to a brief summary of the modern theories of heredity and the rôle and program of eugenics. The negative program, *i.e.* the control and limitation of reproduction among the obviously 'unfit' is advocated as the only practicable one at present in view of the impossibility of determining who are the 'fit' eugenically.

The principles of social groupings are to be found, the author states, in their psychological organization. The rôle of social consciousness and social feeling, of communication, language, culture, and contiguity of various types, are pointed out. Type of organization, the mob, the crowd and higher forms, and types and standards of conduct are considered. The author considers the historical views as to the nature of duty, and accepts a Kantian conclusion that it is 'an ultimate fact' of human life, a specific instance of those awarenesses of relations which are fundamental facts of experience. Generally accepted laws and conventions, with emphasis on the need of conventions to support laws, conclude his treatment of social organization. A chapter on social control, principles of propaganda, and the reduction of the latter to a definite set of rules concludes the volume.

The book contains much interesting and valuable, and some original, material. The treatment of the subject as the application of psychological principles and standpoints to what must necessarily be a brief survey of social life seems far less objectionable to the reviewer than the tendency to draw conclusions of a normative nature and the attempt to play the rôle of a social reformer. A weakness of the book lies in its neglect of an analysis of the social nature of the individual, which is assumed as an ultimate fact along with duty.

Harvard University EDWARD N. BRUSH

Collected Papers, Vol. IV. By SIGM. FREUD. Authorized translation under the supervision of Joan Riviere. The International Psycho-Analytic Library, No. 10. London, Leonard and Virginia Woolf and the Institute of Psycho-Analysis, 1925, pp. 508.

This forms the last volume of the papers thus far published by the founder of psychoanalysis; and for the non-professional reader it is the most

instructive and perhaps the most important of the four volumes. Here we find the groundwork of psychoanalysis represented by such papers as "Formulations regarding the Two Principles in Mental Functioning;" "On Narcissism;" "Instincts and their Vicissitudes;" "Repression;" and "The Unconscious." In this portion of the book, Freud deigns to make indirect references to the physiological mechanism of the unconscious, and perhaps that is the reason why the eight papers of the first part are labelled by the curious name "Meta-psychology." All these essays have appeared between 1911 and 1917, and some have already been widely discussed and reviewed.

The second part of the volume, which is headed "Papers on Applied Psycho-Analysis," deals with the cultural aspect of the subject. Here Freud makes delightful excursions into mythology, drama, sculpture, philology, etc., in order to prove the value of the psychoanalytic method. His "Thoughts for the Times on War and Death" has, it seems, been published in English as a booklet. Of the other essays, the most outstanding are: "The Antithetical Sense of Primal words;" "Some Character-Types Met with in Psycho-Analytic Work;""The Uncanny;""Dreams and Telepathy;" and the three "Contributions to the Psychology of Love," dealing with (a) special types of love-objects, as selected by men, (b) degradation in erotic life, and (c) the taboo of virginity.

It is in these three papers that Freud reveals himself as a sociologist. Nearly every page contains a challenge to the thoughtful reader. Nearly every observation Freud makes stimulates reflection, and nearly every conclusion he comes to sounds labored and far-fetched. At times the author himself is conscious of his failure to convince the critical and avoids the responsibility by statements which smack of an appeal to intuition or to an *ad verecundiam* argument. ("I do not consider it necessary to advance any justification for my method of working out my observations; here, as in the matter of anal-erotism, the aim of it is first of all to single out extreme types in sharp outline" p. 202.)

What these conclusions are, which we take exception to, need not be given here *extenso*, as they are pretty well known. Throughout the book, the incest *motif* plays an important rôle, and from this dominant there develop overtones of a great variety. When he says that "Whoever is to be really free and happy in love must overcome his deference for women," we may perhaps follow him in view of the illustrations cited—cases of psychical impotence—long ago discussed by Krafft-Ebing; but, when he adds in the same breath "and come to terms with the idea of incest with mother or sister," we feel that there is a lacuna in his reasoning.

Freud is a 'pilpulist' *par excellence*. In reach of a simple explanation of a curious fact, he travels to all sorts of regions in order to show the bearing of all his journeying on the theory he wishes to establish. His cultural range is truly to be admired. His genius for discovering problems is incontestable. His invariable return to the subject after considerable meandering is one of the qualities which distinguish him from so many of his disciples. But, with all these qualifications which set him above the majority of mental scientists, his hypotheses still require verification.

Let us take a single instance: Freud comments on the fact that tipplers do not have to change the brand of their wine in order to gratify their craving for drink as do lovers, who soon tire of their sexual objects of love, and he makes this significant utterance with regard to the alleged difference: "However strange it may sound, I think the possibility must be considered that something in the nature of the sexual instinct itself is unfavorable to the achievement of absolute gratification."

Freud does not stop to consider the difference between an *attitude toward an object* like wine and an *attitude toward a personality*. In other words, granted that the difference which Freud has acutely discerned does

hold, we are not justified in believing that a sex object even for a savage is *merely* a sex object. Other factors too must be consulted; and the analogy might be instituted between the object of food and the sex object. Is not the epicure one who would have as great a variety of dishes as possible?

There are many significant passages in this volume. Perhaps the most significant, in view of the fact that Freud is often represented as a foe of every sort of repression, is the following: "It is easy to show that the value the mind sets on erotic needs instantly sinks as soon as satisfaction becomes readily obtainable. Some obstacle is necessary to swell the tide of the libido to its height. . . . In this context it may be stated that the ascetic tendency of Christianity had the effect of raising the psychical value of love in a way that heathen antiquity could never achieve" (p. 214).

Since it has been claimed in some quarters that Freud is inclined to believe in telepathy, it may be pointed out that in spite of the title of the article "Dreams and Telepathy," the author does not commit himself on the subject, and those who can read between the lines will gain the impression that his attitude toward telepathy is one of tolerance.

For the intelligent layman or non-professional psychoanalyst, Vol. IV is the most important of the entire set, and is not only of a wider scope than the other volumes but on the whole brings out deeper issues. The sociologist especially will derive a great deal of benefit from a number of the papers included in the second part. It is here that Freud approaches the world from a phylogenetic point of view; and his speculations on the archaic mind and behavior are interesting even if not convincing.

The English rendering in general runs smoothly, but in some cases it is evident that the translation or revision is not adequate. What, for instance, can one make of the following? "Is it not a fact that wine always affords the drinker the same toxic satisfaction—one that in poetry has so often been likened to the erotic and that science as well may regard as comparable." Nor is this sentence very clear: "Naturally the effect upon a woman is just as unfavorable if the man comes to her without his full potency as if, after overestimating her in the early stages of falling in love, he then, having successfully possessed himself of her, sets her at naught" (p. 211). The use of the word "instinctual" for "instinctive" we may pass over, although it is apt to bother the reader who is accumstomed to the latter form.

Cambridge, Mass. A. A. ROBACK

Fortschritte der Sexualwissenschaft und Psychoanalyse, Vol. II. By WILHELM STEKEL, in coöperation with Anton Missriegler and Emil Gutheil. Leipzig, Franz Deutricke, 1926, pp. iv, 574.

Stekel is the last of the three revolutionaries against Freud to have formed a school for himself; and the bulky periodical of nearly 600 closely printed pages is ample proof that the new school is prospering. Indeed we are told by Stekel himself that his organization includes more professional men than Freud's society in Vienna. There are about 30 articles in this annual besides reviews. The writers are apparently all medical men. Theoretical articles on psychoanalysis are discouraged. For the most part the papers are reports of clinical cases. Only three writers' names are familiar to the reviewer.

Dreams and sex symbolism in general loom large in these reports, which do not lend themselves to reviewing. The most interesting part of the book is Stekel's "Zur Geschichte der analytischen Bewegung," in which he gives an intimate account of the early conflicts in the psychoanalytic camp with details and personal references which make the reader gasp. More than once Stekel asserts that it was he who spread Freud's fame, that Freud was ungrateful to him, that he had put Freud on the right track on

many occasions without the latter having acknowledged his debt. He is willing to admit Freud's superiority of intellect, but insists on exposing his narrow-mindedness, tyranny, intolerance, etc. Stekel's grievances against Freud are many. He also pleads for the other two erstwhile disciples who were lost to the master. Of course he recognizes that Freud can still point to Eitingon, Ferenczi, Rank, Jones, Brill, Sachs, Pfister, van Emden, and Reik; but he rejoins triumphantly: "How do these names compare with Jung, Stekel, Adler?"

Such open frankness may not be without its psychological value. The personal allusions, however, are not likely to raise the reader's esteem of the leaders of psychoanalysis. The impression gained by some will be that they are all a petty band of megalomaniacs lacking taste, breeding, and self-control. Of course this is an exaggeration. Those who have followed the history of the movement know that self-aggrandizement was not the only factor in the various schisms, and that Freud does not hold the respect of so many disciples solely because of the material advantages (recommendations to patients) that he commands. Stekel is apt to do himself more harm by recounting the trivial wrongs he suffered at the hands of Freud (such as coolness, ingratitude) than his alleged enemy, and, even if it is true that "the majority of practitioners come to Freud on account of Stekel," he is not justified in saying about himself in comparison with Freud: "I am the sober observer who draws his compelling conclusions out of clinically observed cases."

Cambridge, Mass. A. A. ROBACK

A Universal Scale of Individual Performance Tests: Examination Manual. By PAUL C. SQUIRES. Princeton, N. J., Princeton University Press, 1926, pp. 158.

In this manual the author presents a scale of individual performance tests which dispense completely with the use of language. He has attempted to construct tests that differentiate even under the extremely difficult situation presented by a superior homogeneous group of adults, though the scale has also been extended downward into the lower levels. In a circular accompanying the manual, Professor C. C. Brigham states, "Mr. Squires objective was that of establishing valid, non-language performance tests at college levels. Some progress was made in this direction." There are fourteen graded problem situations of various types, which may be presented without the use of language.

The apparatus necessary for these performance tests is complex and extensive. This fact in itself should, of course, in no way decrease the value of an adequate scale; but it does present obstacles in the way of validating the material. Detailed descriptions of the test material, of the procedure for administering and for scoring are presented in the manual. All directions are given to the subject in pantomime, so that this scale is in no manner linguistic.

As the author and Professor Brigham point out, the final validation of the scale would require extensive individual examinations over a wide range. Such examinations have as yet not been obtained; and it seems that the procuring of such data will be of unusual difficulty, not only because of the complexity of the test material, but also because of the time required to administer the scale to a single subject; for the period averages about three hours and a quarter (usually in two sittings), with extreme cases running over four hours and necessitating three sittings. Inasmuch as the scale is at present in a tentative state, there are no norms; and naturally so, for at the time the manual was written only a very small number of Princeton students and Vineland school subjects had been examined.

The data thus far collected have been used to calculate validity coefficients. These coefficients are not high, being in the neighborhood of .70. The reliability coefficients (by random halves) is low, being .75. Validity estimates at the feebleminded levels are considerably lower: namely, about .50. These coefficients are not encouraging; but it must be remembered that the scale is not yet in its final form and that the author hopes to stimulate sufficient interest among investigators in the development of the scale to permit widespread application for purposes of validation and standardization.

However, we may question the advisability or the need of developing non-language tests of intelligence to be used at the college level, for the linguistic ability demanded in the language tests is of a nature which any individual in college, or hoping to enter, may be expected to possess without making any undue demands upon his equipment. The advocates of performance tests at all levels make much of the 'school factor' in the tests involving language. Although no one will deny that an individual who has never attend school nor come into contact with the materials of the schools, will be handicapped in dealing with the usual mental tests, yet it has not been demonstrated how potent the school factor is, and how exorbitant in demands on school training these tests are, especially at college levels.

Of course, for purposes of comparative studies of races, and nationalities, a non-language scale would seem to be desirable. But here we meet a difficulty in standardization, for to be a scale truly universal, the norms must be derived and the material must be validated through universal application. The difficulties of obtaining such norms and validation are patent, and we are once more faced with a problem fully as serious as that presented by language in constructing a scale for universal use.

At present the scale presented by Squires cannot be finally evaluated, for it is still in the formative stage. We must have more data and statistical studies.

Cornell University FRANK S. FREEMAN

The Psychology of a Musical Prodigy. By G. RÉVÉSZ. New York, Harcourt Brace and Co., 1925, pp. 278.

The author presents in this book his own translation, but slightly modified, of a work published originally in German under the title of *Erwin Nyiregyhazi* (Leipzig, 1916). "This work," says Révész, "the first of its kind, is an attempt to portray the development of a richly endowed artist from his earliest youth up to a definite period of his existence, to analyze his artistic and intellectual capabilities, and to unite these into an integral whole" (p. vii).

After a brief survey of Erwin's musical inheritance, early musical abilities and training, Révész proceeds with an account of the investigation which he personally carried out upon the artist from the sixth until his twelfth year. Results show that Erwin remained from 2 to 3 years above average in general intelligence, and that in both elementary and complex musical abilities, he was not only richly endowed, but also rapid in his development.

On the basis largely of this investigation, Révész proposes a theory of the general nature of 'musicality.' It is, he holds, a specific, innate entity in the individual's make-up. Musical emotions are likewise specific. In consequence of the specificity of musicality, musical creation is less dependent on general mental development, experience, and practice than is creation in the other arts.

This theory, however, does not seem well supported. In the first place, it was designed to meet the 'fact' that musical creation appears in the

individual at an earlier age than does creation in the other arts. Considering the work of some modern art schools for children, we question whether this 'fact' still deserves the name. Again, Révész frequently makes statements which may easily be interpreted as modifying his theory beyond recognition. For example, the reader is left in doubt as to whether he holds that musicality depends very little, or very considerably, upon general intelligence. A more striking example occurs in his exposition of musicality on its aesthetic side. Musicality, which is "innate," "distinct" and "primarily the ability to enjoy music aesthetically" (p. 22), nevertheless, he claims, "finds expression . . . also in the course of many other activities which do not stand in any . . . immediate relation to music" (p. 23 f.). Without good evidence, which in this case is lacking, it seems arbitrary, in the face of this last statement, to call the ability to enjoy music aesthetically the *primary* characteristic of musicality; indeed, it seems extremely arbitrary to attribute to a particular musicality any of the activities in which it is said to manifest itself. Finally, Erwin's case does not seem to point to Révész's conclusions. From his theory alone, one might infer that Erwin possessed, besides his great talent for music, only the most ordinary equipment. On the contrary, however, he was above the average in intelligence; he was rich in his general emotional nature, and he had excellent musical training.

On the whole, it seems that though many sound observations are made by the author, they do not well support his central doctrine of the specificity of musical ability.

Vassar College HELEN K. MULL

La psychologie de la conversion chez les peuples non-civilisés. By RAOUL ALLIER. Paris, Payot, 1925; Vol. I, pp. 595; Vol. II, pp. 509.

This rather formidable but very interesting study of the conversion experience among primitive peoples is replete with illustrative examples drawn from missionary activities in all parts of the world. Attacking the subject systematically, the author considers the psychological problems involved in the prodromal stages of the experience, in the experience itself, and in the conditions following the crisis.

An extended discussion of the obstacles which the missionary meets in his very ambitious attempts to affect an almost complete change in the orientation point of the beliefs and conduct of the native is presented. The hostility, the language handicaps, the differences in mentality, the apparent propensity for absurdity shown by the native (M. Allier explains this by a 'paralogic' theory rather than by Lévy-Bruhl's 'prelogic' theory) are among those he takes up. The problems and mechanisms involved in the resistances offered by established habit systems, in the superficial assent often obtained, and in the tendency for social conformity, are pointed out and methods of overcoming these difficulties discussed. An examination of fatalism and magic and the more elusive and subconscious factors which slowly relate the primitive self to the new viewpoints is followed by a discussion of the more immediate elements, such as dreams and hallucinations, which enter into the conversion experience.

The actual crisis raises the problem of the reorganization of the dissociated personality about a new point. M. Allier considers the various aspects of the situation, discussing the "death" involved and the emotional phenomena which accompany conversion. He defends the theory which considers emotion as the essential phenomenon in the conversion against that which sees in it merely a means of provoking the crisis.

In the last section, devoted to the social and individual consequences of the conversion, he enters into a lengthy discussion of the psychological factors involved in such problems as the relationship between the changed

individual and the environment, the battle with negative and formalistic morality, discipline, the change of habit systems, and the gradual development of autonomy.

Except for occasional notes of religiosity which mar the study, these volumes are a genuine contribution towards the understanding of the innumerable problems of primitive mentality and religious psychology. They should be of value to students of anthropology and abnormal psychology as well as to those interested in religious phenomena. They might well serve as handbooks for the missionary.

Harvard University DAVID SHAKOW

Ventilation: Report of the New York State Commission on Ventilation. By C. E. A. WINSLOW and others. New York, E. P. Dutton & Co., 1923, pp. xxvi, 620.

While the present report raises a number of ventilation problems needing further investigation, it is undoubtedly the most elaborate study of this important problem that has been made in recent years. There is not space here to present more than a few of the more significant conclusions. (1) The data show that "the effects of chemical impurities in the air of the ordinary occupied room are of relatively slight importance in their influence on health and comfort, and that the main objective of ventilation is the preservation of the proper degree of temperature, humidity, and air movement which will facilitate the elimination of heat from the body surfaces and will stimulate those surfaces by an agreeable degree of coolness and air movement." (2) No significant differences in physical defects between groups exposed to air with an average relative humidity of 28.7 per cent and 42.3 per cent were noted. In other words "artificial humidity does not produce any striking improvement in the health or efficiency of school children." (3) The use of recirculated air reduces fuel cost materially, shows no harmful effect upon the health or mental processes of pupils, but does cause marked feelings of discomfort. (4) Over-heating is one of the greatest dangers to be avoided. (5) The needed air change may fall materially under the 30 cubic feet per occupant per minute as required by present day standards. (6) Finally "we are forced to conclude that window ventilation, with ample direct radiation, window deflectors and adequate gravity exhaust seems the most generally promising method for the ventilation of the classroom where local conditions permit its use." The slower air movement and the lower temperatures characteristic of this method as compared with a mechanically forced supply are important factors in this conclusion. At the same time it must be recognized that this method may not give best results where noise and dust are bothersome or where rooms are crowded as in an auditorium or a factory.

Cornell University JULIAN E. BUTTERWORTH

Our Minds and Their Bodies. By JOHN LAIRD. London, Oxford University Press, 1925, pp. 122.

The author has designed this little book for the consideration of the mind-body problem at different levels of discussion. He begins with 'the attitude of common sense,' which he says has been predominantly and persistently dualistic. He defines common sense as the trend of current opinion, untrained and uninstructed. To guard against any materialistic interpretation of his accepted form of dualism, he defines spirits as "things invisible and intangible and in no sense a subtle breath or dry tenuous flame-auras and ectoplasma."

The author's interpretation of mind is essentially animistic, and much of his discussion is given over to a criticism of purely behavioristic and mechanistic conceptions. He does not, however, agree with MacDougall's animistic conception which he holds is too similar to the Scholastic inter-

pretation. Apparently Laird believes mind to be, not of bodily origin, but simultaneous in its origin and development with that of its body. "It seems easier," he says, "to suppose that the roots of man's spirit are not to be discovered in particular human minds, and so the disparity between spirit and matter implies the belief in some cosmic order of spirituality as natural as our minds and as real as our bodies." "It is easy to speak," he continues, "like some late-born Averroist, of a spiritual radiance, broken up, colored and particularized by the prisms of our bodies. This is to assume, however, that men's differences and their personal characteristics are fundamentally bodily. On that theory, men *are* what their *bodies* can attract and re-fashion from spiritual springs; and this quite precisely is *not* what we take our spirits to be. For we believe that although we may be sustained and aided from sources not our own, and perhaps neither mundane nor human, our principal task in this mind-body partnership is to develop our minds in accordance with their own self-accepted standards, and to use our bodies as the mind's servant and friend."

Harvard University C. A. SELZER

Personality. By R. G. GORDON. New York, Harcourt Brace and Co., 1926, pp. xiv, 302.

Personality is considered by the author from the standpoint of the 'identity hypothesis.' Physiological processes have their psychological correlates, but unfortunately, he neglects the conscious correlates on account of his emphasis upon the physiological side. The spiritual aspect of personality he adds at the end as if to counteract the accentuation of the physical that runs through most of his discussion.

Physiology is thus the dominant note. The study of temperament includes an enumeration of glandular activities with some slight mention of the psychological consequences involved. Food metabolism furnishes the only energy which acts within the personality. Talents, intelligence, and genius are functions of cortical integrations. Thought is a new emergent from eccentric combinations of impulse, depending upon inhibitions and facilitations.

The book covers too much ground. Personality is delineated in terms of *Gestalt*, engrams and engraphic patterns, relatedness, and emergence. Personality, like consciousness, is an emergent, for it "has no meaning in terms of nature alone, nor yet in terms of nurture alone, but it is an emergent from the interrelatedness of the two." Man is destined to emerge on the level of Deity.

The review of the psychoanalytic schools is concise and to the point. On other topics the author is less coherent. For the reader who wants a bird's-eye view of selected high spots in psychology, with physiological speculations to lend color to the treatment, the book would hold an appeal.

Harvard University W. F. VAUGHAN

Motion Pictures in Education. By DON CAROLOS ELLIS and LAURA THORNBOROUGH. New York, Crowell Co., 1923, pp. xvii, 284.

This volume is primarily a practical handbook for users of visual aids. As such it gives specific information regarding the types of school work in which films may be profitably employed, types of films available for instruction, and suggestions as to how they are to be used. There is also a chapter on the technique of installing and operating the necessiary equipment. A part of the book is devoted to the history of education with regard to visual aids and an analysis of objections to and advantages in the use of films for instructional purposes. The results of several experiments on the relative effectiveness of different methods of presenting materials

are given. In one test, abstract and concrete subjects were taught by (1) film alone; (2) a superior teacher; (3) an average teacher. The scores were 75.5, 66.9, and 61.3. Another test gave lowest score to the lesson presented by the teacher alone, next lowest score to the study of the printed lesson, third lowest to the film alone, and highest to the film accompanied by explanatory comments. The authors' generalization from these and similar data is that "while the majority of tests are favorable to visual education and may prove helpful and should therefore be encouraged, they are not conclusive unless supported by reason and sound pedagogical principles." While the book is frankly a defense of teaching by means of moving pictures, it gives the impression of being a restrained and well-balanced presentation.

Cornell University JULIAN E. BUTTERWORTH

Special Disabilities in Learning to Read and Write. By E. LORD, L. CARMICHAEL, and W. F. DEARBORN. Harvard Monographs in Education, Series I, Vol. II, No. 1. Cambridge, Mass., Harvard Graduate School of Education, 1925, pp. 76.

This monograph is a report of two case studies—one on mirror-writing by Dr. Carmichael, and one on congenital word blindness by Miss Lord—with a critical survey by Professor Dearborn of the important literature on word blindness. The survey justifies the theses offered in the studies that both disabilities are due to inhibiting habits based upon specific intellectual shortcomings and are not due to congenital defect.

Miss Lord bases her conclusion on the fact that a boy, once diagnosed as feeble-minded, showed normal reading ability five years after she gave him a period of intensive training in reading by all of the recognized methods. She also refers to other similar instances which one would like to analyze more thoroughly before accepting the generalizations to which she commits herself.

Dr. Carmichael's study of mirror-writing agrees with Ladd's claim that this disability originates in extreme left-handedness, but attributes the phenomenon to the trial and error scribble period of the process of learning to write, when the separate hit-and-miss responses made by the left-handed child are gradually integrated into wholes of the mirror-script rather than of the normal type.

Harvard University H. MOORES

Pleasure and Pain: A Theory of the Energic Foundation of Feeling. By PAUL BOUSFIELD. New York, E. P. Dutton & Co., 1926, pp. 114.

Bousfield's general hypothesis is that all life is tension, and that pain is the affect accompanying tension. All stimuli raise the tension above the normal level and therefore tend to cause pain. Pleasure is the affect accompanying a discharge of tension. Pain and pleasure are not opposed under this hypothesis but may exist simultaneously in the organism. Fear is conscious fore-pain or the anticipation of tension.

The author abandons the two primitive instincts of self preservation and reproduction, and substitutes for them the instinct for the avoidance of pain (tension). This instinct serves incidentally to preserve life. A second instinct is assumed: that by which the organism seeks to reduce tension to a minimum. The end would be death, could it be attained without tension or pain. Continual fore-pleasure serves as a control to the complete functioning of this instinct.

The libidinous instinct of Freud is not regarded as purely sexual. The energy of the libido may arise from any somatic discharge of tension and is sexual only when it flows through sexual channels.

The author's final suggestion is that there may be but one basic tendency in both living organisms and inanimate matter. All substance is in a state of tension and the basic tendency is towards a reduction of this tension.

Harvard University — MORGAN UPTON

Industry and Civilization. By C. DELISLE BURNS. New York, The Macmillan Co., 1925, pp. 273.

In this book, Burns discusses the ethical principles implied by conduct in different fields of economic life. The ethics of labor, in his view, is social and class-conscious; of managers, individualistic and optimistic; of capitalists, cautious. No clear distinction is made by the author between 'good' and 'right' as applied to conduct. Judging the ethics of different classes with reference to a more ultimate standard, which he assumes to be a 'form of harmony,' Burns advocates a gradual extension of coöperation in industry.

Opposing the view of the utilitarians, among whom were numbered the classical economists, that the sole or chief motive is pleasure calculated in units, the author attributes behavior to 'the horme' which is creative and expresses itself in instinctive behavior. He indicts the minute division of labor in modern industry because it prevents adequate instinctive activity through making remote the end of labor.

Consumption is regarded as social, since no person consumes in society what he would consume if isolated, even with the same range of products. In the sense of coöperation, Burns writers of 'mind groups' which he appears to favor, thus assuming a position related to that of political pluralism.

Harvard University — J. C. KELSON

Pluralistic Philosophies of England and America. By JEAN WAHL. Translated by Fred Rothwell. London, The Open Court Co., 1925, pp. 324.

This is an important and timely work, being a brief history of the new significant movements in philosophy in England and America. Mainly it is a history of pluralism, the new, democratic, social philosophy made famous by its chief American representative, William James. Incidentally, it is a history of all the more virile movements in recent American and English thought, treating of the closely related philosophy of pragmatism and explaining the background in English, French and German thought from which both philosophies sprang. Considerable space is given to Ménard and Renouvier, to Bradley and Bosanquet, to Russell and to the American neo-realists and critical realists, to James most of all and incidentally to all the lesser lights, especially in American universities. If the study is carried on in "a patch work, piecemeal fashion," this method the author thinks was advisable. A thoughtful evaluation of pluralism forms the concluding chapter.

University of Iowa — G. T. W. PATRICK

Experience and Nature: The Paul Carus Foundation Lectures. First Series. By JOHN DEWEY. Chicago, Open Court Pub. Co., 1925, pp. xi, 443.

In this book Professor Dewey has made a metaphysical analysis of his views. It is a systematic development of the metaphysical implications of non-reflective experience. The book holds rich rewards for the student of philosophy, but the psychologist will find little material directly bearing on his field.

NOTES

Some Difficulties with Weiss's Behavioristic Postulates

In a recent volume Professor Weiss proposes first to establish behaviorism upon certain postulates of physics and then to make it a basis for the social sciences.[1] Having definitely committed himself to a given set of postulates he can scarcely deviate without subjecting himself to the criticism of internal inconsistency which less explicit authors happily escape. It appears to me that Weiss has failed both to establish a sound theoretical basis for his doctrine, and also to remain faithful to the postulates which he has espoused. His distrust of metaphysics has not led him to master the technique of that art.[2] Furthermore, we may question whether he has really made his science, as he professes, "a study of the material, biological, mechanical, and social antecedents that are at the basis of human achievement" (259).[3] Let us consider certain fundamental difficulties both with the theoretical basis and also with the 'psychological' structure reared thereon.

I. Criticism of the Physical Postulates

(*A*) *The physical postulates are inadequate.* Weiss proposes to take as his basis electrons and protons (or whatever ultimate units the physicist might postulate) and from them to work out a complete psychology in quantitative terms of motion (389). This does not mean that the behaviorist deliberately ignores what is traditionally known as psychological material, such as sensations, images and emotions; instead he proposes to improve the explanation of these materials (vii). The 'psyche' and the sensations into which it is analyzed Weiss claims are but assumptions like those of the ether or the electron (44, 53, 62). They are unnecessary, however; and, moreover, they have not maintained their status in the other sciences (68, 72). They merely complicate the psychological problem because they are undefinable and immeasurable (71, 282). Thus confronting himself with two sets of assumptions, the physical and the psychical, Weiss chooses the former (34, 47, 49, 63, 70, 138 f.) The assumptions of 'physical monism' have two advantages over psychical assumptions; first, the unit of explanation, *i.e.* stimulus-response, is composed of two variables of the same entity (motion) which can accurately be measured; and, secondly, this type of explanation is common to all the physical sciences. Electrons and protons can be synthetized to atoms, molecules, protoplasms, animals, man, and even social organizations (44-45, 20, 47, 391). "The individual is the locus in the movement continuum" (392, see also 52, 142). "Human behavior and achievement are assumed to be forms of motion" (389, also 138, 141). On the other hand, the "*ultimate realities* of the professional metaphysicians seem incapable of synthesis into anything more unified than is implied by the term *uniqueness*, which can neither be demonstrated nor defined" (45).

It is difficult to understand why Weiss should set up such obviously artificial and restricted alternatives. He appears naïvely to believe that if he accepts experiences, he is bound, then, immediately to reify them into psychical substances and to regard them as a 'soul;' but that if he rejects experiences he is left with the single alternative of electrons and protons. It seems never to have occurred to him that our experiences do not neces-

[1] A. P. Weiss, *A Theoretical Basis of Human Behavior*, 1925. See also his article, One set of postulates for a behavioristic psychology, *Psychol. Rev.*, 32, 1925, 83-87.

[2] See, for example, Weiss's reply to A. E. Davies (Mechanism, meaning, and teleology in behavior, this Journal, 37, 1926, 2-24) A rejoinder, *ibid.*, 450-459, 628.

[3] The page references in the text refer to the *Theoretical Basis of Human Behavior* except where otherwise specified.

sarily imply such an entity as 'mind.'[4] Most psychologies of the past generation have no such 'psyche' and yet they treat of empirical and demonstrable phenomena. Electrons and protons, on the other hand, are constructions which physicists have inferred in order to explain certain observed facts within their realm. I say 'certain' because the physicist has so defined his ultimates as to apply only to his physical problems.[5] This behaviorist, then, rejects data common to all science for a set of limited physical assumptions.

(*B*) *The physical postulates are tacitly rejected.* Weiss seems to have been aware that such categories as "stimulus character (meaning) of responses" (56), "the biosocial response which exists in a number of biophysical variants more or less interchangeable" (330), and "social status" are not describable in terms of his physics. He makes provision for a new basis of measuring human achievement, with the following apology.

"Because of the fact that *time, place,* antecedent social conditions, play a much more important role in the changes that take place in man than they do in inorganic changes, new classifications in which man is regarded as a co-operative unit in a relation of sensori-motor interchangeability with all the other members of the community, will need to be invented before the fundamental units of physics . . . . will be fruitful" (395, also 46, 54-55, italics mine).

It is hardly necessary to point out that here the postulates of physics are surreptitiously discarded. Weiss's excuse for throwing out the physical standards of measurement is "the fact that time, space, [and] antecedent social conditions play a much more important role" in psychology! But are not time and space measured physically in terms of centimeters and seconds or of the speed of light? It is not these concepts which Weiss would reformulate for psychology, but the third, "social condition" which, being independent of the specific neuromuscular items involved, is not described in physical terms.

(*C*) *The meaning of the terms 'External World' and 'Response' is ambiguous.* There could be no objection to a behaviorism which consistently followed its postulates and definitions, and accepted with good grace the limits of those postulates. But Weiss does not heed the boundaries, playing fast and loose with his electrons and protons and stretching the elastic term 'response' until it extends beyond its legitimate physical interpretation.[6]

(1) The concept of the external world is used ambiguously.

(*a*) The external world is a fiction. "Of course, the electrons and protons are metaphysical constructions which belong to the same category as 'thing-in-itself,' neutral stuff, élan vital, psychic force" (44).

(*b*) The external world is visible electrons and protons. "For the physicist, then, the electron-proton hypothesis is a guess as to what would be observed visually, with a microscope of sufficient magnifying power" (41).

(*c*) The external world is a human response. "Reality is merely the term that designates this type of relationship between responses. It is the basis of the fiction of an external world of stimuli" (43).

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[4]This was pointed out, ineffectively it seems, by Davies (*op. cit.*) and constitutes the essence of W. B. Smith's criticism of Weiss (Motion? or emotion? *Psychol. Rev.*, 33, 1926, 159-165).

[5]"Physics . . . . is unable with its limited intellectual implements, created for limited special purposes, to exhaust all the subject-matter of science." Ernst Mach, *Contributions to the Analysis of the Sensations*, 1897, 1-2.

[6]Considering this matter in another field, Robert Lowie remarks, "From one angle it might seem that a man has a right to define his terms as he will, provided only he adhere to the usage once established, ... This position is inexpugnable so long as it is avowedly no more than a personal evaluation; it is shattered as soon as it pretends to give an objective classification of the pertinent data . . . . it recalls that comical classification of some aboriginal language by which male members of the tribe are of one gender, while all other persons are lumped in a vast complementary category together with animals, plants and objects" (*Primitive Religion*, 1924, xi).

(*d*) The external world contains the qualities of experience. "To have the *sensation of blueness*, requires at least: (a) the spectral blue stimulus, (b) the specific retinal elements" (272).

"The traditional psychologists would affirm that there is in this so-called experience [perceiving a square of red paper] a quality of *redness* which is not in the stimulus, not in the sensori-motor conditions, not in the reaction, as reaching nor in the verbal naming of the color" (266, also 267, 251, 270 f.).

"In their complex forms stimuli form the objects, situations, and events and the records of the objects, situations, and events, which form or have formed the environment" (134). "The problem stimulus" (323). "The mother stimulus" (340).

(*e*) The external world is composed of movements known as light, sound, etc. "The electron-proton movements may be grouped into various geometrical types. Some movements, as those occurring in a luminous body, are transmitted enormous distances and at a rapid rate, as in light, magnetic, and electrical waves. Others are . . . . vibratory motions in air particles, as in sound waves, etc." (26-27).

The external world then is (*a*) a metaphysical fiction, (*b*) electrons and protons that might be seen, (*c*) my response, (*d*) the source of 'secondary qualities,' and (*e*) the traditionally called 'adequate stimuli.' These are certainly contradictory assertions; but it is obviously convenient to have so many places to put 'sensory qualities.' For example, by putting colors into the stimulus ("the spectral blue stimulus") he answers to his own satisfaction Professor Washburn's criticism that behaviorists explained sensational qualities by attributing them at times to the stimulus and at other times to the response (271 f.). As if to justify Professor Washburn's criticism, the qualities previously were "discriminatory responses . . . making up the environment" (197-199).

(*2*) The term 'response' is inflated to universal proportions. The response, or human behavior, is variously conceived as:

(*a*) Movements between electron-proton systems.

"Thus, in the last analysis human behavior is reduced to movements between electron-proton systems" (36).

(*b*) Contractile effects.

"A response is a unified group of contractile effects which as movements form the basis of the co-operative, receptor-effector (sensori-motor) interchanges between individuals. (The response begins with the chemical change in the receptor, and ends with the contractile effects)" (143).

(*c*) Bodily movements which serve as stimuli.

"For the behaviorist life, action, and the so-called psychical conditions can only be adequately studied as movements (in contractile elements) of such magnitude that they may act as stimuli and release responses in others" (152).

"The actual neuromuscular factors that are involved in human behavior are relatively unimportant as compared with the *stimulus character of the response for other individuals*" (102, also 188, 273, 289).

(*d*) Sounds, written characters, printed symbols, or meaning.

"When I respond to the voice of my fellow man I react to the series of sounds that he produces" (288).

"Differentiation can only mean that *two* different sets of movements have been made: (*1*) a subject has said *e.g.* 'this is red,—this is green,' and (*2*) an observer has recorded the movements of the subject" (195-196).

"Thus there are two criteria with respect to which human movements may be classified: (a) as neuromuscular effects of *preceding* movements, (b) as neuromuscular causes of *subsequent* movements . . . when a

reaction is classified with respect to its stimulus character (meaning) either for other individuals or for the same individual at a later time" (55-56).

In this last case the "movement," as a stimulus for the same individual at a later time, seems to be printed matter.

(*e*) Experiential qualities, which in turn make up the environment.

"Those discriminations that have a high degree of uniformity between various individuals I have arbitrarily designated as elementary discriminations. . . . The following list contains the important elementary discriminations: size, shape, volume, distance, extent, intensity, direction, red, green, blue, yellow, vocality, tonality, loud, warm, cold, pressure, pain, sweet, salt, sour, bitter, fragrant, foul, resinous, burnt, fruity, spicy, movements of the body or parts of the body, up, down, right, left, posture, hunger, thirst, fatigue, duration. This list is not complete. It represents merely the type of responses that are relatively uniform from individual to individual. In combination these discriminative responses make up the objects and events of the individual's environment" (197-199).

(*f*) *Everything* is a response.

"Consciousness as an implicit form of behavior or as an obscure physico-chemical process is best described as behavior . . . the writer has made several attempts to carefully work out the behavior equivalents of the more important traditional psychological terms, but implicit reactions and subvocal speech seem to explain everything" (253).

"Thinking is the stimulus-response relation, but so is every other form of human behavior" (331).

"All human conduct and achievement reduces to *nothing but*:

(a) different kinds of electron-proton groupings . . . (b) the motions that occur when one structural or dynamic form changes into another" (50).

"Reality is merely the term that designates this type of relationship between responses" (43).

"For the behaviorist the concept of the *self* is a specific response" (203). "The solution response" (329). "At ease response" (338). "Snuggling response" (340).

Although it is good nineteenth century materialism to reduce everything to 'nothing but' something else, it remains true that what we perceive is precisely what we perceive. Furthermore, with Weiss the term 'response' has become almost as general as the term 'being.' The explanation, then, that "this is a response" has practically no significance, since no distinction is made. The differentiae are to be considered in the following sections.

II. Criticism of the Psychology

But Weiss is writing a psychology, and it is only fair that this psychology be considered apart from the more or less irrelevant and labored 'physical foundations.'

The chief criticism which our author makes of the traditional psychology is that sensations, images, and conscious states will not explain human behavior. These may be all right for picturing a cross-section of an individual; but a cross-section tells nothing about its origin or future (240). It leaves out of account the functional continuity. Psychology "must assume he [man] is the product of biological, anthropological, and social antecedents" (4, 388). Accordingly, the behaviorist proposes to investigate the ontogenetic and phylogenetic development of the educational, vocational, administrative and other activities of the individual" (191). Psychology is to be the link between the physical and biological sciences, on the one hand, and the social sciences, on the other.

(*A*) *The arbitrary concept of movement is not followed.* The first difficulty lies less in aim than in method. Weiss proposes to ignore the 'cross-sections' entirely, and to consider the genetic aspects of human behavior in terms only of physical movement. But one may well ask, upon what is the concept of "movement" based?

"All definitions are self-limiting and based on other definitions. The fundamental assumption of all science presupposes that individuals are able to understand each other, and that the limited range of definition may be supplemented by demonstration. Such fundamental concepts as movement, space, time, are best presented as a demonstration" (20-21).

Aside from the fact that it is not the concepts of movement, space, and time which he demonstrates, but only objects or events which appear to move, to have extension, and to endure, we may well ask why he does not demonstrate colors, sounds, odors, images? When it is dark one can no more see movement than one can see shadows, as Jurgen observed. All are equally dependent upon an intact organism. Weiss has rejected them, however, on the basis of his physical postulates, which, as we have seen, make a distinction between primary and secondary qualities. By granting 'reality' to movement only Weiss has ruled out all descriptive psychology. Those phenomena, which, like colors, images and odors, are not movements, cannot be taken at their face value, but have to be defined in terms of movement.

"The image is rather the functioning of internal and rather obscure motor patterns which were at one time released" (256, also 234).

"For the behaviorist, sensations and images must be *movements* and have specific sensori-motor configurations" (265).

Now if what is traditionally called blue is for the behaviorist but an obscure motor process, what is this process? If it is what non-behaviorists call kinaesthesis that is another 'sensory quality' of the same order as the visual blue. For the behaviorist it must be *another* obscure movement. However, if the traditional blue moves, the behaviorist as well as the non-behaviorist can directly observe and demonstrate that.[7]

The question now arises as to how these other phenomena are defined. Although everything that cannot be demonstrated as movement is to be defined in its terms, the definitions do not take the form merely of quantitative differences. That is, there are qualitative aspects which do not appear in the postulates. The obvious criterion, the movements of the various muscle-groups or neuromuscular patterns, is the one which Weiss starts out with (36, 56, 143, 152). We might expect that these "internal rather obscure motor patterns" (256, 271), which constitute the images and sensations, would play a large part in behavioristic psychology. Indeed, as Davies pointed out, only so long as Weiss keeps the term 'response' limited to the contractile elements does he follow the method and point of view of physics. But the bare movements are rejected; although all things may be only movement, the significant thing for psychology is not what the movements are. What is important is the stimulus character of the responses for other individuals. "The actual neuromuscular factors that are involved in human behavior are relatively unimportant" (102, also 142).

"The individual is classified not on the basis of physical and physiological properties but on the basis of his co-operative status in the social organization of which he is a unit" (142, also 275 ff., 309 ff.).

Since a number of biophysically different actions may be biosocially equivalent, and biosocially different acts may be biophysically equivalent (287-333), socially significant 'responses' cannot be differentiated according to the antecedent neuromuscular factors involved, but according to their

[7] In man we have a condition in which an organism "*responds to its own responses*" (255).

results in establishing the individual's social status. Weiss builds a psychology by ignoring all demonstrable phenomena save movement, and then loses interest in the movements as such. We shall now consider the 'movements' as biosocially significant.

(*B*) *'Social Status' is inadequate as a differentiating principle.*

The analysis of human behavior considered biosocially may be outlined as follows:

(1) "Human behavior is the totality of the response systems that establish the individual's status in the social organization of which he is a member" (143, also 35, 133, 191, 207-222).

(*a*) "The study of human behavior and human achievement is a study of the educational, vocational, administrative, recreational and personal categories of movement" (394, also 36, 135 f., 143, 191, 209 ff., 388). "This qualitative analysis is to be subdivided under two heads: (*1*) the degree of excellence or skill, and (*2*) the degree of co-operative activity in social organization" (216 ff., 394).

Participation in golf, for instance, would fall under 'recreation.' Within this class all individuals would be ranked in accordance with their excellence or skill. This constitutes the quantitative aspect of the behavioristic program, involving tests, correlations, and distribution curves. Finally, the coöperative activity which the individual displays in social organization is somehow to be given an index number. To aid in this analysis Weiss converts such concepts as language, thinking, desire, purpose, volition, motivation, and emotion into the "major response categories" (287-383).

The definition of human behavior as those response systems that establish the individual's social status falls short because it fails to take account of the conventionally organized responses, morality, tradition, and human institutions. These have elsewhere been behavioristically defined.[8] If it be argued that "the totality of the response systems" refers to these antecedent facts, the answer is that the five-fold analysis of behavior into educational, vocational and other activities is then incomplete on two accounts, since it refers only to the individual behavior. (a) There are a great many things besides the individual's behavior which establish his social status. In many countries social position is inherited. The social organization of India, and perhaps of China, does not give the individual much opportunity to establish his social status. That type of movement which establishes an individual's status in such a society where he is born into his position could scarcely be imputed to the innocent offspring. (b) The corollary of this is that an individual's acts are in part a result of his social status, and not always a cause.

Although Weiss set out to include an ontogenetic and phylogenetic account of the development of responses (191), the classification he suggests is again a cross-sectional picture, and leaves out of account the social determination of acts.

(*C*) *The classification of human behavior is sociological.* The primary difference among the sciences depends upon their points of view. The chemist, for example, views water as the compound H_2O; the geologist may consider it as an erosive agent; the physicist considers it as a fluid of a definite specific gravity. The science of psychology likewise has a definite point of view. It "seeks to describe and to understand experience and the activities of the total organism in which experience plays an essential part."[9] The analysis of human behavior into educational, vocational, administrative, recreational, and personal activities neither explains nor describes the individual's performance; it gives neither functional nor

[8] S. Smith and E. R. Guthrie, *General Psychology*, 1923, 220-239.

[9] Madison Bentley, *The Field of Psychology*, 1924, 15.

structural differences in the individual acts which get themselves thus classified. These classes are not ways in which the organism functions. Weiss has given a classification of human behavior from the standpoint of social organization, that is to say, a sociological classification.[10] Psychology has its own definite problems and point of view which are not to be confused with those of other sciences; nor is psychology the handmaid of the social sciences.

SUMMARY

This paper has attempted to show that Professor Weiss's system of psychology based on the postulates of the physical sciences encounters difficulties and inconsistencies.

I. With the physical postulates:

(*A*) The assumed postulates of the physical sciences are inadequate by definition as the basis for psychology;

(*B*) Weiss tacitly admits the futility of these postulates; and

(*C*) He maintains the appearance of consistency by using ambiguously the terms "stimulus" and "response."

II. With the psychological structure:

(*A*) The concept of movement is taken arbitrarily, and then discarded;

(*B*) The differentiating principle, establishing psychology as "those responses establishing the individual's social status" is inadequate; and finally

(*C*) The basis for the classification of behavior is sociological, not psychological.

University of Illinois RALPH GUNDLACH

EMPIRICAL PSYCHOLOGY

Professor Carmichael has recently discussed the different meanings of the term "empirical psychology" and has advocated the use of but one of the three meanings which he finds.[1] Our choice of meanings, he says, lies among the following. (1) "Empirical" may be equivalent to "experimental," for an experiment by nature is empirical: it makes use of experience by way of observation. (2) "Empirical" may mean "genetic," and more particularly "ontogenetic," as when one opposes an 'empirical' to a 'nativistic' theory, especially in theories of perception. (3) Or "empirical" may be used to indicate whatever it was that Brentano meant in his *Psychologie vom empirischen Standpunkte*, a use that Titchener has made of the term in describing the antithesis between Brentano and Wundt.[2]

This third meaning Carmichael finds vague and ambiguous. "Empirical" and "rational," he thinks, are the antonyms as applied to method. Brentano, he suggests, uses a rationalistic method, and in that is not empirical at all; but Brentano also goes to observation and experience, and to experiment where it is available, and to this extent (if I understand Carmichael aright) Brentano is an experimentalist. Thus it would seem that the third meaning either is a contradiction of terms or reduces it to the first, and may thus be dispensed with. The second meaning may, according to Carmichael, also be discarded. It is clear and distinct from the first, but it is never desirable to have two meanings for the same word. For the second meaning he thinks that the word "genetic" is adequate and in accordance with modern usage. Thus we may well, the entire argument runs, equate "empirical" to the first definition.

By this process of elimination, Carmichael is convinced that "empirical" and "experimental" should be equated, and that a fiat should issue thus restricting the polygamous term. It is my own impression that this ar-

[10] J. R. Kantor also makes this point, *J. of Philos.*, 24, 1927, 22-25.

[1] L. Carmichael, What is empirical psychology? this JOURNAL, 37, 1926, 521-527.

[2] E. B. Titchener, Brentano and Wundt: empirical and experimental psychology, *ibid.*, 32, 1921, 108-120.

gument tends rather to abolish, than to define, the term 'empirical.' 'Experimental' is a fairly precise and univocal term; why still risk confusion by preserving two words, which, being two, imply a distinction, when their meanings are identical?

Such at least would seem to me to be Carmichael's proper conclusion, but it is not my own. I have tried during the past year in a course of lectures to see if I could proceed without the word 'empirical,' and I have not been successful. My consequent dissent from Carmichael is on two grounds. (a) I believe that Carmichael has overlooked a fourth meaning for 'empirical,' where the word carries a real distinction. (b) Hard and fast definition by fiat seems to me undesirable.

(a) 'Empirical' seems to me to be simply the adjective for 'experience,' the usual substitute for the more awkward 'experiential.' (Cf. the forms 'empiricism' and 'experientialism.') 'Experience' and 'experiment' are, however, far from being interchangeable terms. The one is the underlying substrate of science and of knowledge. The other is, even in its broadest meaning, a particular method. The method of experiment is nevertheless dependent upon experience; it is a method for getting experience to yield the generalized relationships which we call facts. It is a method of control by the repetition of isolated events with predetermined variations. It is Mill's method of concomitant variations in the concrete and in the laboratory, with the entire stellar universe as a possible laboratory. I am not seeking rigidly to define an experiment; I am merely indicating the degree of its particularity. Every experiment is 'experiential' and hence 'empirical;' but all 'empirical' methods are not necessarily experimental.

The philosophy of nature and, to a less extent, natural philosophy are empirical, but they may not be experimental. The modern philosopher who writes psychology with little or no recourse to psychological experiment may be empirical.[3] Like the experimentalist he goes to experience for his basal material, but it is to casual observation or his own personal experience that he goes and not, very often at least, to experiment. It was in this sense that Herbart wrote a psychology "gegründet auf Erfahrung" but not on experiment. Lotze's psychology was similar. I have always supposed that it was this philosophical, non-experimental streak in Brentano that Titchener had in mind when he distinguished between Brentano and Wundt as empirical and experimental psychologists.

It is doubtless this philosophical bent that Carmichael is thinking of when he hints that Brentano's psychology might be called "rational." I question, however, the feasibility of carrying this distinction through. Experiment is certainly not irrational: it is inductive logic put into the concrete, and usually artificially controlled. Moreover, both Brentano and Herbart are speaking of "Psychologie als Wissenshaft," and we cannot deny that it is from experience that they proceed by the rationalistic method. If experience is fundamental to Brentano's psychology, even though the experience does not, and sometimes can not, be made to, conform to the experimental method, shall we deny him the title of "empirical psychologist?"

I think then of 'empirical psychology' as something broader than 'experimental psychology,' something which includes 'experimental psychology.' In such a sense it cannot be opposed to 'experimental psychology.' Nevertheless, when the opposition has been explicitly formulated, as it has by Titchener, I believe that no confusion results. 'Empirical psychology,' as opposed to 'experimental psychology,' is simply that portion of experiential psychology that is neither *a priori* or wholly rationalistic on the one hand, nor experimental on the other.

That empiricism leads more readily to rationalism than does experimentalism seems to me to be simply an exigency of system-making and

[3]Almost any philosopher-psychologist will do for illustration; I might cite W. E. Hocking, *Man and the State*, 1926, 199-336, and especially 199-218 to the psychologist who likes to see how little the philosopher thinks the psychological experiment can accomplish.

science, but it is a useful relationship to know. To say, for instance, that Wundt's tri-dimensional theory of feeling was in the first instance 'empirical' and not 'experimental,' is at once to suggest to the wise that it depends upon casual and individual observation and to hint that emphasis upon the rationalistic method is likely to be found.[4]

(b) My other dissent from Carmichael lies in his attempt to control the use of the word "empirical." Let us have clearness in scientific writing, but let us gain clearness simply by being clear. In writing, it seems to me, we use, not words, but language; and the attempt to secure clarity by lawful definition as often defeats itself as it accomplishes its end. Those who write words, rather than sentences, are forever coining new terms and limiting old in order to conduct their arguments. It is seldom that these terms gain scientific acceptance; all one is apt to have is difficult exposition with obfuscation corresponding to the difficulty. But let me be concrete.

I seem to be saying that 'experimental' is the word for Carmichael's first meaning and 'empirical' for the third, and thus to be implying that "genetic" or "ontogenetic" is the term for his second. What then about 'English empiricism?' Perhaps, if my meaning for 'empirical' be accepted, it is still some kind of 'empiricism;' that all knowledge comes out of experience was Locke's fundamental thesis. Nevertheless the important characteristic of this school is that knowledge develops from and by experience; it provides a genetic theory of knowledge. Shall we turn to "English geneticism"[5] as a better term? Scarcely, for the meaning of the usual phrase is too well established; we can not violate history and tradition so cavalierly.[6]

What then about theories of perception? Shall we speak of 'nativistic' and 'genetic' theories but not of 'empirical' theories? Here I quite agree with Carmichael that the word 'genetic' is ordinarily clearer. Wundt's theory of space perception seems to me not to be greatly distorted when it is described as 'genetic,'[7] especially where his emphasis is on the development of the perception of space and not on its origin in experience. Helmholtz, however, preceded Wundt in his theory, and I should still like the freedom to call his theory 'empirical.' If one reads for example his chapter "Concerning the Perceptions in General,"[8] one sees how different is his interest. He emphasizes not the process by which visual perceptions are developed (*der unbewusste Schluss*), but the variations of perceptions from the central tendency established by the sensory stimulus in so far as these variations can be referred to the experience of the individual (and sometimes of the race). To call Helmholtz' theory a 'genetic' theory is first to miss the proper emphasis, secondly to avoid the correct implication that the theory is the direct outgrowth of English empiricism, and thirdly to be historically inaccurate where accuracy is easy.

We are all grateful to Professor Carmichael for clarifying the several meanings of the word 'empirical.' All I ask further of him is that he leave us free to use words as they may best be used in different contexts, insisting as much as he pleases that we be clear, but letting us try to achieve clarity without restrictive legislation as to definitions.

Harvard University EDWIN G. BORING

[4] Titchener, *loc. cit.*, remarks that Brentano's psychology is largely a matter of "argument," Wundt's of "description;" but this is not to say that Wundt was never 'argumentative' nor Brentano 'descriptive.'

[5] Or "English ontogeneticism"!

[6] I should suppose that Carmichael, having equated "empirical" to "experimental," might speak of "English experimentalism" for Locke and his successors, if he were emphasizing, not the development of knowledge, but the notion of the *tabula rasa*; but such a result would surely be a *reductio ad absurdum*.

[7] Nevertheless Wundt consistently writes "*empiristisch*:" cf. W. Wundt, *Grundzüge der physiologischen Psychologie*, II, 1910, 710-716. For his use of "genetisch" and "Entwickelung," see p. 717.

[8] H. L. F. v. Helmholtz, *Handbuch der physiologischen Optik*, 1866, sect. 26 (or in the later editions or the Eng. trans.).

The 'Range of Attention' Experiment

Certain differences of definition are evident with regard to experimenters working on the range of attention problem. I am particularly interested in the series of papers by Gill and Dallenbach,[1] and by Oberly.[2]

Dallenbach and the writer would seem to be in complete accord in regard to a number of points—primarily that the older examples of the 'range of attention' experiments (and Dallenbach lists 10 from Hamilton, 1859 to Kraskowski, 1913,) cannot possibly be considered to have attacked the problem of attention. This was my point of view in 1921, when I published on the subject.[3] Because of the complexity of the material and because of the form of the report, the experiment was most certainly on the cognitive and not on the simple attention level.

As a result of this opinion, Oberly approached this type of problem using simple stimuli and employing a report of the number of elements for each exposure card. He further required a characterization of conscious process following each exposure. These characterizations were based on a preliminary introspective study in which it became evident that there were three conscious patterns which might underly the type of judgment required. These were (1) an immediate process in which the elements had an equal clearness; (2) an immediate process in which the elements were grouped or did not have an equal degree of clearness, and (3) a conscious process that involved some mediating reimaging (counting). Oberly then statistically combined his results, calculating the limens for (1) alone; for (1) and (2); and for (1), (2) and (3). These he called respectively 'attention,' 'cognition,' and 'apprehension' and they became experimentally defined as follows: 'attention' is a process which is immediate and involves only one degree of clearness; 'cognition' is a process which is immediate but which may involve one or more degrees of clearness; 'apprehension' is a process which may either be immediate or mediated by reimaging. This last was most certainly what had been the basis for all former range of attention experiments.

I am sure that Dallenbach and Oberly and the writer would agree that the last two—'cognition' and 'apprehension'—are merely different degrees of cognition based upon different degrees of complexity of the mental process. But I am sure that Dallenbach and I would disagree on the first—Dallenbach would insist that this again is a lower degree of cognition while I would insist that it is as close as one can get to experimentally working with attention.

Dallenbach would emphasize his contention by quoting results of the study by Gill and himself—a study in which the stimuli were the usual ones presented to the *Os* in the range of attention experiments and his *Os* were merely asked to report if the material was of a single, dual, or multiple level of clearness. On the basis of these results, these investigators concluded that the range of attention (defined in terms of sensory clearness) was from 17 to 42 elements.

The writer is completely unable to comprehend these findings—primarily on the basis of a single criticism. He does not question that all of the elements seen were at a single level of clearness if they were so reported. But what assurance did Gill and Dallenbach have that the *Os saw* all of the elements on a given card. This is apparently assumed. It has been our experience that, with exposures as rapid as 60 sigma, many elements may be

[1] N. F. Gill and K.M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 247-256.

[2] H. S. Oberly, The range of visual attention, cognition and apprehension, *ibid.*, 35, 1924, 332-353; The range of visual attention, cognition and apprehension experiment, *ibid.*, 37, 1926, 132-138.

[3] S. W. Fernberger, A preliminary study of the range of visual apprehension, *ibid.*, 32, 1921, 121-133.

on a card which are not seen at all. My own and Oberly's findings indicate that this happens when stimulus cards contain only 3 or 4 elements. With cards as complicated as those containing 10 or more elements, this failure to see all of them was common rather than the unusual state of affairs.

The situation seems to be something of an impasse unless one accepts Oberly's definition of attention. Otherwise one must accept Dallenbach's experimental definition which is perfectly workable from the qualitative point of view. When one attempts a quantitative determination, however, one must have a check that *all* of the elements on a given card were in consciousness. Such a check involves either a report of number or of position or of something of the sort—all of which involves cognition and therefore raises the experience above the simple attentional level as Dallenbach defines it.

The writer is personally convinced that Oberly's experimental definition of attention as an immediate process all of the elements of which have an equal degree of clearness is as close as one may get to an experimental quantitative attack on the range of attention; and the writer is not sure that Oberly's experimental defintion of Cognition as an immediate process in which the elements appear in consciousness with either equal or different degrees of clearness is not as close as one may come to an experimental attack on the problem of the range of two level or multilevel attention. Both definitions imply the immediacy of the conscious process and one implies the equal clearness of the elements.

University of Pennsylvania SAMUEL W. FERNBERGER

DR. FERNBERGER ON THE 'RANGE OF ATTENTION' EXPERIMENT

Through the courtesy of Professor Fernberger, I have the opportunity of answering his Note, so that the readers of the JOURNAL may have his criticism and my reply in the same number.

The disagreement between us is a curious one. We are agreed upon all the experimental facts, but we are at odds regarding the terms that should be used in their discussion. The facts are these.

I. The so-called 'range of attention' experiments that have been undertaken during the past 100 years have been concerned with the determination of the number of objects that can simultaneously or successively be correctly apprehended. The justification of designating these experiments as 'range of attention' experiments rests upon two assumptions: (*1*) that all processes attended to are reported; and (*2*) that all processes reported had been attended to. In an experiment upon "Attributive *vs.* Cognitive Clearness,"[1] I discovered (1) that neither assumption was correct: clear processes are not always reported, nor had the processes reported always been clearly perceived; and (2) that many other factors besides clearness of experience condition the report.[2] I therefore felt justified in concluding: "the statement in current textbooks that the 'grasp of visual attention covers from four to six simultaneously presented simple impressions' is a statement concerning visual apprehension, not attention."[3] In a study made approximately at the same time, Fernberger reached similar conclusions. "From a consideration of the introspections we are convinced," he says, "that the range of attention is an erroneous title for this sort of experiment."[4]

II. The next step was taken by Oberly[5] who, working under Fernberger's direction, "attempted to differentiate the thresholds of attention,

[1] K. M. Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 183-230.
[2] *Ibid.*, 213-233, 228 f.
[3] *Ibid.*, 229.
[4] S. W. Fernberger, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 121-133.
[5] H. S. Oberly, The range of visual attention, cognition and apprehension, this JOURNAL, 35, 1924, 332-353.

cognition and apprehension.'"[6] He followed the traditional procedure in his experiments, but in addition to the report of (*1*) "the number of dots apprehended," he required his *O*s to give (*2*) their "degree of assurance or certainity," and (*3*) "the method of determining their report."[7] His *O*s, under (*3*), differentiated three conscious patterns, which he characterized as "immediate," "grouping," and "counting."[8] Using the reports that were correct, and that *O* was 'sure enough about to put even money upon,'[9] Oberly computed, by the method of constant stimuli, limens for every one of these categories.

There is no disagreement between Fernberger and me regarding Oberly's data. I have, now that Oberly has pointed them out, repeatedly observed the three conscious patterns that he has described. Of Oberly's experimental findings there is no doubt; of the interpretation of his data, however, as much cannot be said. Because the characterizations 'immediate,' 'grouping,' and 'counting' did "not correspond to the systematic categories that have appeared in literature," Oberly 'transformed his results' into attention, cognition, and apprehension.

He transformed the 'immediate' pattern into 'attention' because his *O*s' description of this pattern "agrees with the definition of attention as given by Titchener;"[10] the 'grouping' pattern into 'cognition' because "Dallenbach found in an experiment on sensory clearness that two levels were possible, and . . . [that] the experiment dealing with two such levels is an experiment in cognition,"[11] and also because Dallenbach, "associates the 'lower level' of clearness with cognition;"[12] and the 'counting' pattern into 'apprehension' because "Fernberger has so defined the range of apprehension."[13]

As I pointed out in a note published in criticism of Oberly's paper[14]—to which, by the way, Fernberger make no reference—the transformation of his first two patterns is not justified and Oberly misunderstands both Titchener and me. Attention for Titchener is a conscious pattern that is divided into the clear and the unclear.[15] It is not an cognitive term. No one, not even the functional psychologists, regards attention as a level, a step, or a degree of cognition, and yet that is just what Oberly makes of it when he designates the 'immediate' pattern as attention. His *O*s must *cognize 'number'* before giving a report. His experiments were therefore upon the cognitive level.

Curiously enough, Fernberger makes me "insist that this [Oberly's 'immediate' pattern] is a lower degree of cognition."[16] Quite on the contrary, I believe that it is the highest degree of cognition with which Oberly was concerned. Oberly's results, as do the results of all the 'range of attention' experiments, show that the constant varies with the degree of cognition. The higher the degree of cognition, the smaller the constant; the lower the degree, the higher the constant. When the *O*s, for example, are required to name the stimuli exposed the constant is about 5; when they are required merely to report the number exposed the constant is about 9.

III. Cognition is inherent in report, and the degree of cognition required for the report determines the range of the report. It may very well be, therefore, that the question of range is not a proper one to ask regarding

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[6]*Ibid.*, 333. [7]*Ibid.*, 335. [8]*Ibid.*, 333. [9]*Ibid.*, 336. [10]*Ibid.*, 332, 335, 347. [11]*Ibid.*, 348. [12]*Ibid.*, 333. [13]*Ibid.*, 350.

[14]Dr. Oberly on 'The range for visual attention, cognition and apprehension,' this JOURNAL, 36, 1925, 154-156.

[15]Cf. E. B. Titchener, *The Psychology of Feeling and Attention*, 1908, 220 ff.; *A Text-Bookof Psycholgy*, 1909, 266 f.

[16]*Vide supra*, 478.

[17]N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 249.

attention. Nevertheless Miss Gill and I thought that by restating the question so that the problem became one of discovering how many stimuli may be placed upon an exposure card that should correlate 50% of the time with one level of clearness, and that by requiring the *O*s to report, not upon the stimuli themselves, but upon the distribution of the clearnesses of the impressions, we should be able to eliminate the cognitive complication, or at least to reduce it to a minimum, and thus obtain a constant that would be significant for attention.[17] Our study led to positive results: "under the conditions of our experiments the individual limens varied from approximately 17 to 42 stimulus objects."

Fernberger says that he is "completely unable to comprehend these findings" because he finds no assurance "that the *O*s *saw* all the elements on a given card."[18] Truly, we have no such assurance, and what is more to the point we did not care whether our *O*s saw all the elements, half of them, or only a small fraction of them. All the elements had the same opportunity of being seen; beyond that our interest did not go. The question whether all the items were seen no more concerned us than the question whether all the grams in a 100-grm. weight were sensed would concern Fernberger in a lifted-weight experiment. The method of constant stimuli, as Fernberger knows, is adequate to such subjective variables. What we were solely interested in was to discover, by the method of constant stimuli, the number of dots that would correlate 50% of the time with one level of clearness.

Fernberger is no more interested than we are in the number of dots that the *O*s saw. Both he and Oberly had to face the same problem in their studies upon range, and they solved it in exactly the same manner that we did—by computing their limens by the method of constant stumili. They both used in their computations only the reports that we were objectively correct. If, for example, an *O* reported 8 dots when a stimulus card of 10 was exposed, it was counted as incorrect, although, as Fernberger points out, 8 may have been the actual number seen. It mattered not at all to Fernberger or to Oberly how many dots were seen; the thing that interested them was whether the report was correct. So also with our work: it mattered not to us how many items were seen; the only thing that we were interested in was whether the report was of one or more levels of clearness.

Oberly is explicit in his definition of attention, and it may seem to many as though he should have the right, as long as he is consistent in his usage, to define terms as he will; but until he can point to a system of psychology (or give us one) in which attention is regarded as a level or degree of cognition he is not justified in altering the accepted usage simply to classify his own data. To do so only compounds the confusion which already exists.

Cornell University KARL M. DALLENBACH

THE TWENTY-SECOND MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The twenty-second meeting of the Southern Society for Philosophy and Psychology was held in the University of Georgia, Athens, Georgia, April 22 and 23. This is the home of the president of the Society, A. S. Edwards, who had laid careful plans for the meeting and had arranged to have exhibits of psychological apparatus by C. H. Stoelting and Company and of standard tests by the World Book Company, and a preschool exhibit by B. T. Baldwin (Iowa). After a short address of welcome by Chancellor Snelling of the University, the program was begun by a report on further experiments on the problem of sleep by H. M. Johnson (Pitts-

[18] *Vide supra*, 478.

burgh). By means of his well known methods of recording bodily movements of subjects during the hours of sleep he had found that with better sleep at night there was associated a loss in efficiency in the substitution test the next morning, but that on the following evening efficiency in this test was increased. The curves of shifts in position during the night show not only a rapid decrease in such activity soon after going to sleep, reaching a minimum point at a little over one hour and thence rising gradually toward the hour of waking with acceleration toward morning, but also somewhat regular rhythms in amount of movement throughout the night. These rhythms hold a somewhat constant relation to the time of going to bed. One experimental group put to bed earlier yielded results showing that these rhythms shifted forward in correspondence with the earlier time of going to sleep.

R. M. Dorcus (Hopkins) reported the results of experiments on the effects of fixation of the head upon post-rotation nystagmus time as contrasted with non-fixation. Ten subjects were used of whom five were rotated with heads free and five with heads fixed. The latter group showed the greater reduction in nystagmus duration after rotation. G. C. Bassett (Kentucky), who had observed his own delusions during delirium in a spell of illness, reported the fulfillment in the delusions of certain childhood desires which had been denied expression. The desires to have roller skates, suppressed by parents because of fear of danger to the head, to join the navy and become a sailor, and to build railroads and become a train dispatcher were appropriately provided for in the dreams of the delirium. Subject found himself skating with head protectively wrapped in a pillow to prevent injury; he experienced sailing dreams involving capture by pirates and later escape; and recognized himself as building and operating railroad systems. He contended that these dreams were not of a sort demanded by the Freudian doctrine; sex drives were entirely absent and dreams were expressions of unfulfilled wishes rather than of suppressed desires. It developed in the discussion of the report that since these wishes were often indulged in during childhood years, the dream revivals of the earlier pictured fulfillment may have been shaped in a measure by frequency laws and intensity of image; also that the memory report after the delirium might be influenced in a degree by selective factors emphasizing content that had obvious relations to past experiences. Buford Johnson (Johns Hopkins) discussed the activities of two-year-old children as observed in the new child laboratory under her direction. Forty children of one and one-half to three years of age had been under observation both while restrained under certain test conditions and while unrestrained. These children were found to indulge in activities mainly of speech and of motor coördination. It was reported that delay in the development of these motor coördination activities lead to later lack of social adaptation. Studies of problem children also revealed early hamperings of these childhood activities which normally establish basic coördinations. In the discussion that followed it was admitted that the relation between hampered early activities and later mal-adaptation was not established as a causal one, but that both conditions may in part at least be results of some common factor. Even if this should prove to be the case, however, it would not negate the importance of the early activities.

Joseph Peterson (Peabody College), ending the forenoon program, gave a report as Chairman of the Committee on the Status and Teaching of Psychology. Mimeographed tables of the qualifications of 302 teachers of psychology and of psychological books and journals available in 158 institutions in sixteen Southern states were distributed for study and discussion. It was shown that the more foreign to psychology the training of persons actually teaching this subject had been the more willing they seemed to be to undertake such teaching with limited academic training.

Two-thirds of the 158 institutions reporting had only two or less than two psychology journals available to students. The committee emphasized the need of campaigns to increase the number of psychology journals and books in the numerous small colleges now offerring work in this line. A mimeographed list of all coöperating institutions in the sixteen Southern states, which reach the standards outlined by the Society in its 1926 meetting was also presented for discussion and correction. The institutions were arranged alphabetically by states, and specifications were made as to whether all courses, or only elementary courses, met the standards. In cases where some, but not all, instructors were properly qualified the work of those instructors only was approved. In the discussion it was brought out clearly that there may be serious injustices in the rating of instructors merely on the basis of their academic training or degrees and on published work; but it was agreed that with due care these injustices could be largely avoided and that a published list of the kind presented, distributed to university registrars and to professors of psychology in higher institutions of learning would do much good toward the prevention of transfer of credits in the case of unsatisfactory work. The work of the committee was thought to be invaluable toward the establishment of better standards of psychology work. The list of institutions meeting the standards of the Society will be worked over thoroughly and published in due time. It is important that all qualified institutions be on this list.

The afternoon program was devoted to philosophy. A. O. Lovejoy (Johns Hopkins) read a paper on emergent evolution. The roots of present views were traced and it was asserted that emergent evolution theories are neither affirmations nor denials of possibilities beyond our present realm of experience. We must be open to facts as they come. Important questions are what sort of new events appear and how they come. Emergences may be either functional (novel manifestations of old types of events) or existential (real additions to what actually existed before). Just what constitutes new events as distinct from merely new manifestations of old events is a question not easy to solve and one on which different answers are given by different authorities. Some writers maintain, for instance, that only psychical, or conscious events are but new mainfestations of old events. This is the question as to whether dualism can be established on a factual basis. The paper was too detailed and replete with content for adequate summary and may profitably be read in full by psychologists. In the discussion Lovejoy held (against Sanborn) that prediction is possible without quantitative analysis. C. G. Thompson (Emory) as to the status of the 'value judgment' held that philosophers used to regard value judgments as having reference only to ultimate realities, but admitted that, like everyone else, they have found it difficult to get beyond their world of experience. All knowledge is an organization of events in our world. If our knowledge does not reach reality, we have no knowledge; were this true we could not even think. Religion has been as agnostic as philosophy, looking beyond the world for reality. The universe is really coherent and in it (not outside of it) all things have their place. To the question, has the autonomy of the individual anything to do with his behavior? Thompson answered, the individuals' response might be different if his organism were different. E. H. Henderson (Georgia) gave an exposition of Vaihinger's philosophy 'As if,' holding that this instrumental view was not to be identified with pragmatism. Since this was an exposition only it is unnecessary to summarize here. H. G. Sanborn (Vanderbilt), discussing methodology and psychology, held that science has to do only with quantitative prediction. Science is not, as is often claimed, without presuppositions; every science begins with assumptions which it does not and as science cannot investigate, and it ends with results which it as science cannot evaluate. Practical science simplifies the complexity

of the world as presented empirically and renounces knowledge in order to predict. All methods must be either mechanical in the mathematical sense or vitalistic. The only evolution we understand at first hand is psychic evolution; mechanical evolution is a contradiction in terms. If there is real evolution it is not mechanical. All evolutionary doctrines represent an attempt to extend a truly functional or psychological method downward, as the mechanistic method has been extended upward from physics to history. There is no valid objection to an extension of either of these methods into the realm of the other as all methods are relative to their purposes. Lack of time prevented the discussion of this paper which was scheduled.

In the presidential address, given in the evening after the dinner, Edwards reviewed the various definitions of intelligence and added one more, the capacity for variability, or versatility, of response. He did not bring out the common ground in many statements of what intelligence is, and neglected, unfortunately, to include with his own theory, not a new idea so far as variability is concerned, the fact of some sort of organizing function.

In the Saturday morning program R. L. Bates (Virginia Military Institute) discussed, in connection with the presentation of data which he had gathered, the complexity of factors leading to the withdrawal of students from college. These factors were shown to operate in various combinations making prediction of an exact sort difficult. Joseph Peterson (Peabody College) presented data from experiments on the effect of practice on individual differences. It was shown that correlation methods alone were inadequate here, and that the coefficient of variability, though not without weaknesses, was indispensable. An historical survey showed that most practice effects in different kinds of performances had resulted in the subjects' becoming more alike, but that in certain complex motor coördinations, like ball-tossing, there was wide divergence with practice. An experiment on twenty 12-year-old children in Peabody College gave divergence in substitution practice and in mental multiplication, and convergence in complex card sorting. Extended to 96 subjects of a more heterogeneous nature—one of the Peabody experiments applied to Idaho children by M. C. Barlow—the mental multiplication practice yielded convergence. It was found that training in most school subjects, including psychology, also gave decrease in the variability coefficients; this, however, may be accounted for in part by the fact, brought out in data from Myers Mental Measure on large numbers of children of ages from 6 to 14 years, that marked convergence in variability of mental test scores through successive years occurs. Studies extending through a considerable length of time may be influenced by a natural convergence of individual differences with age. P. F. Finner (Florida State College) followed with a paper on the variability of individual performance. Using a variety of performance tests he obtained wide ranges of variability coefficients on the same individuals in different performances, varying from .07 to .18. He interpreted this wide variation as due to the fact that some processes practiced were newer to the individual than the others. The variability coefficients of the several individuals regularly decreased with practice, a fact, he pointed out, which does not contradict any of the results of the previous paper which dealt with changes in percentage variation in groups. It was maintained that the variability coefficient was significant and that every student should know not only his level of performance in different activities but also his degree of variability in performance. No relation was found between the level of efficiency in performance and the variability coefficient.

W. W. Reitz (Georgia) showed, in carefully prepared charts of students' average college grades as related to their intelligence test performances, that several factors lowered the correlation. Students with high abilities as revealed by the tests were able to get along in their college work with lit-

tle effort while in numerous cases those scoring lower had worked hard to compensate and had consequently made great gains in scholarship during the year. Lines connecting the ranks of individual students in intelligence tests on entrance with rank in college subjects at the end of the year crossed very much, connecting high points on one side with low ones on the other. Interpretation was, however, confused with facts, the author assuming too much for the accuracy of placement as to ability by the intelligence tests and explaining divergences between scores and college grades as due to differences in effort, a point to be established. The results were admittedly marred by the fact that the different instructors assigning the grades had no uniform system of grading. Paul Boynton (Kentucky) presented results of a moral judgments test, which he rightly distinguished from a test of morality. The test was his own construction. Applied to college students it showed a certain reliable sex difference as to standards regarding the use of stimulants, and the appropriation of the property of others, but differences in judgments as regards the relations of the sexes were not reliable. S. C. Garrison (Peabody College) reported on the reliability and validity of certain group intelligence tests. Two hundred forty-nine children in the Peabody Demonstration School, distributed through grades of 4 to 10 had been given a number of different intelligence tests. These tests were administered at year intervals during a period of 5 years. Every child was thus tested 6 times but, due to irregularities in attendance, no one particular test was given to all the children. In all, 10 tests were administered and the smallest number of children taking any one test was 66. As a measure of the validity of any test the following criteria, weighted equally for a composite score, were used: The Stanford-Binet I. Q., a composite intelligence test score, teachers' judgment of ability, and a composite educational test score made from 37 different measurements, each of these also being given equal weight. The results of the correlation processes placed the National and the Terman tests at the top in reliability and validity, both for an interval of one year and of five years. Dearborn Series II and Haggerty Delta 2 ranked at the lower extreme. E. Bagby (North Carolina) analyzed two cases of simple hysterical reactions pointing out what he called both functional and organic factors. He emphasized the importance of showing in detail what all the contributing factors of such reactions are, rather than simply attributing the finally resultant reactions to a disease entity 'hysteria.'

At the business meeting Saturday morning Virginia Military Institute, Lexington, Virginia, was designated as the place for the 1928 meeting and Josiah Morse (South Carolina) was elected president for the next year. New council members elected were L. R. Geissler (Randolph Macon Woman's College) and R. L. Bates (Virginia Military Institute). A motion by Lovejoy empowering the new president to appoint a committee on the teaching of philosophy was passed. Considering the fact that the meeting was held in the far South the attendance was good. A number of the west Southern states were, however, not represented.

Peabody College JOSEPH PETERSON

THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The second annual meeting of a group of midwestern experimental psychologists was held on May 14, 15, in Wieboldt Hall on the McKinlock Memorial Campus of Northwestern University at Chicago, Illinois. The program opened on the afternoon of the first day with a session which was devoted to a discussion of the content and subject matter of psychology and its relation to the other sciences. The most important changes in psychological theory and practice during the last twenty-five years were also described and some ideal experimental programs for future investigation

were suggested. Professors D. T. Howard (Northwestern), C. L. Hull (Wisconsin), M. F. Meyer (Missouri), Karl Schneider (Wittenberg), L. L. Thurstone (Chicago), and A. P. Weiss (Ohio State) and others participated in the discussion. After the informal discussion the group was pleasantly entertained at tea in the rooms of *The Monist*.

In the evening the chairman of the group, Professor A. R. Gilliland, introduced Dean G. F. Arps, who gave an address on the subject of laboratory psychology, urging more interest and attention to a thorough-going laboratory course in psychology as a preliminary to all further research as well as an integral part of the elementary course. After the address a committee was appointed to consider the question of organizing the group on a more permanent basis. Professor Gilliland appointed Professors Weiss (chairman), Carr, Hull, and Ruckmick to consider this question and to report.

The next day there were two concurrent sessions during which about a dozen papers were presented on experimental results and interpretations. In one session presided over by Professor J. J. B. Morgan the following papers were presented:

M. Metfessel, Phonophotographic Studies of Negro Singing;

S. R. Hathaway and J. P. Porter, An Improved Psychogalvanic Apparatus and Preliminary Findings;

C. W. Darrow, The Significance of Blood Pressure and Galvanic Changes in the Study of Emotions;

H. M. Johnson, Some Further Experiments Bearing on the Problem of Sleep.

At the other session, at which Professor C. A. Ruckmick presided, these papers were offered:

H. Woodrow, The Effect of Duration of Limiting Sounds upon Estimates of Short Temporal Intervals;

A. G. Bills, The Influence of the Quantitative Set upon Initial Efficiency

E. Culler, Nature of the Learning Curve;

E. Jacobson, Productivity of American Psychologists;

Valentine, Visual Perception in White Rats.

Later in the morning opportunity was afforded for inspection of the medical laboratories and of the law building in the new imposing units recently erected on this downtown campus. Afterwards Professor L. L. Thurstone read a paper on: A Criticism of the P.E. Unit in Scale Construction.

In the afternoon, which was a four-hour session, Professor C. L. Hull who presided called for reports of research that were being carried on at the various laboratories represented, including the University of Chicago, Clinic for Juvenile Research at Chicago, University of Illinois, Indiana University, University of Iowa, Iowa State College, University of Minnesota, Ohio State University, Ohio University, Ohio Wesleyan University, Washington University, University of Wisconsin, and Wittenberg College.

In the evening of the second day a banquet was held in the rooms of the Women's City Club at which President Walter Dill Scott was toastmaster and Professor H. A. Carr read a paper on 'Teaching and Learning.' In this paper he showed by means of experiments on white rats and on human subjects that under the conditions of the procedure often guidance in learning was less effective than punishment, but that in other series too much guidance was less effective than none at all. At this banquet, attended by approximately seventy-five psychologists, the report of the committee mentioned above was read and approved.

This report provided for a permanent organization known as the Midwestern Psychological Association, composed of those members and associate members of the American Psychological Association who have

registered at any annual meeting of the former organization. Professor A. R. Gilliland was elected president for the current year and an executive council was provided for composed of Dr. C. L. Hull, Dr. M. Metfessel, Dr. S. Renshaw, and Dr. L. L. Thurstone. The duties of the president and the executive council were covered in the by-laws which were adopted. It was decided that the organization meet next year at the University of Wisconsin. Appreciation for the unusual facilities and courtesies provided were expressed by a rising vote of thanks.

The meetings were unusually well attended by about two hundred psychologists and guests and the papers were in the main of an experimental nature based upon data either in process of investigation or obtained from recently finished work. The papers are too numerous to be reported in detail, but the more valuable ones will undoubtedly be published in due course.

University of Iowa CHRISTIAN A. RUCKMICK

TRENDS IN CHINESE PSYCHOLOGICAL INTEREST SINCE 1922

The trend of psychological interest in China since 1922 is clearly shown by the type of the articles published in *The Chinese Journal of Psychology* that was founded in January 1922 by the Chinese Psychological Society. The *Journal* is edited by the president of the Society, Professor Y. C. Chang of Peking National Normal University. It is published irregularly, but has now reached its fourth volume.

The following is a complete list of the subjects of the articles which have appeared in the *Journal* up to and including the March 1926 number. The classification is the original one adopted by the *Journal*. Most of the articles are elementary introductory discussions.

Abnormal. History and content of abnormal psychology, psychoanalysis (2 articles), dreams (2 articles), interpretation of dreams, arguments against the existence of prophetic dreams, criticisms against Coriat's theory of the interpretation of dreams, Freud's dream theory.

Adolescence. Juvenile crime (2 articles), sex problems, sex psychology of Chinese students.

Aesthetics. Translation of article on the psychology of music by J. E. Weathery.

Applied. Principles of applied psychology, physiognomy of Chinese handwriting, work and efficiency.

Animal. Psychology of crow, pigeon, ant; animal intelligence, recognition of nest-mates by ants, red and anger in domestic animals, animal instincts.

Biography. James, Wundt.

Business. Psychology of Chinese merchants as seen from 5000 signboards.

Child. Methods of studying children's knowledge; color appreciation; learning to reason; suggestibility in children; interests of Chinese children; fear, interest, and the moral sense; history of child study.

Educational. Origin of educational tests; tests for recognition of Chinese characters; experiment on Chinese phonetics; reading test; Chinese philosophers on human nature as good or bad; American standardized educational tests; a test of educational conditions in South Manchuria; measurement of elementary school handwriting by T. scale; three types of educational scales; science test for secondary schools.

Experimental. Geometrical illusions, conditioned response, mirror drawing, eye movements in reading.

General. Work and fatigue; psychology of fancy; names for noises found in Chinese literature (2 articles); mental imagery of several great Chinese writers; instinct (3 articles); attention and interest of Chinese

poets (2 articles); genesis of emotion; a statistical study of the colors in national flags of the world; 203 special names for colors found in Chinese literature; 267 more color-names in Chinese literature; words and thoughts; first memories in life; self and soul; emotion in Chinese new poets; preference in the use of the ten figures; hair-fetishism and brow-fetishism; Henning's experiments on smell; the attention value of Chinese sparrow-cards; behavioristic interpretation of thinking; a new interpretation of the formation of concepts; classification of laughter and its physiology; anger; mathematical method of the associative inference of psychology.

Heredity. Transformation of silkworm eggs into goldfish (2 articles).

History. Of psychology (4 articles); modern psychological movements; evolution of modern psychology; latest developments of Russian psychology; German schools of psychology.

Intelligence Tests. Origin, history, use; Courtis arithmetic test; definition and limit of intelligence; experiment on "pa-kua;" Japanese adaptation of the Binet-Simon tests; a list of American group tests; the "fruit test;" elementary school common-sense test; intelligence tests and education; Chinese group tests; national intelligence tests of Japan; translation of article by Pintner on comparison of American and foreign children in intelligence tests.

Psychological terms. Standardization of Chinese terms in psychology.

Psychological tests. An emotion test; psychological tests found in China during the Han and Wei dynasties (206 B. C.–264 A. D.).

Religion. Child's conception.

Senile. Learning capacity of an aged person.

Social. Test of people's opinions (2 articles); Chinese superstitions (3 articles); a statistical study of Chinese words expressing virtues and vices (2 articles); correlation of name and fame; geographical, educational, and vocational distribution of 750 leading contemporary Chinese; geographical distribution of the graduates of the Imperial Academy of Learning during the Ching dynasty (1644–1911); the psychology of fortune-telling.

Systematic. Buddhist psychology; introspective *vs.* behavioristic psychology; Wang Yang-Ming's psychology (1472–1528); life of Freud and progress of his thought; Chu-tse's (1129–1200) and Hsin-Tse's (about 600 B. C.) thoughts on psychology.

Vocational. A vocational study of Peking Middle School seniors; vocations preferred by 850 Chinese school children.

Stanford University SIEGEN K. CHOU

WILLIAM ALLEN NEILSON CHAIR OF RESEARCH

The establishment of the William Allen Neilson chair of research at Smith College, Northampton, Mass., has recently been announced. The chair is the gift of friends and admirers of Dr. Neilson in honor of the tenth year of his presidency of Smith. It was presented by Mr. George Bliss McCallum of Northampton, a member of the Board of Trustees, speaking for the donors.

The terms on which the chair is established are somewhat unusual, since the holder is not expected to teach and is left absolutely free to study and experiment as he pleases, publishing or not as he prefers.

The chair will be held for five years by Professor Kurt Koffka of the University of Giessen, Germany.

Edward Bradford Titchener